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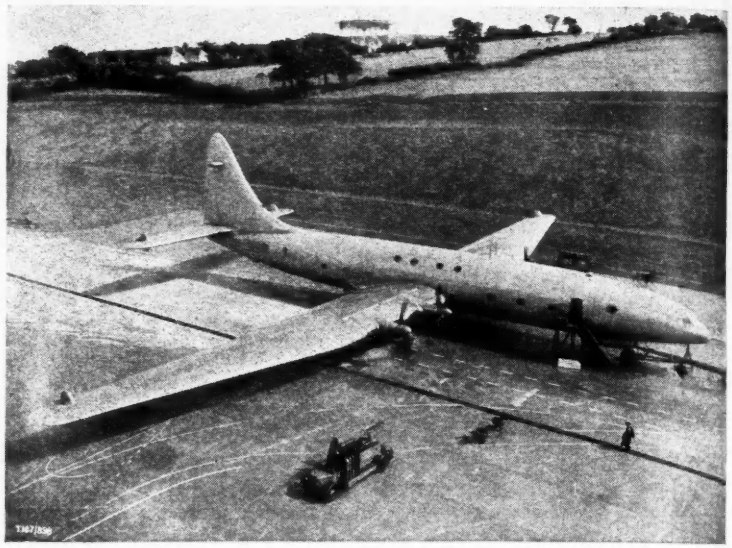

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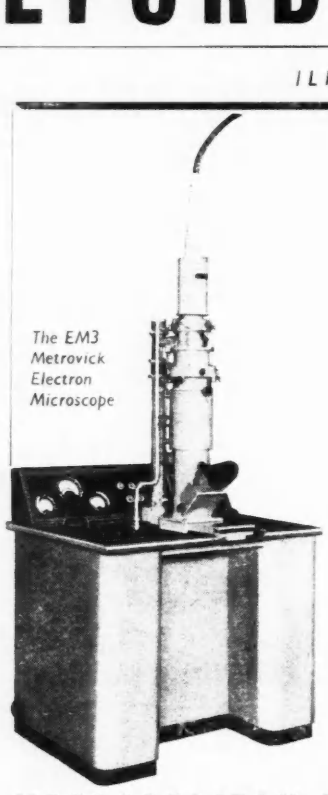

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DISCOVERY

THE MAGAZINE OF SCIENTIFIC PROGRESS

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The Progress of Science

Cortisone

THE remarkable effect of cortisone in the treatment of rheumatoid arthritis recently announced from the Mayo Clinic has been acclaimed, even by the usually phlegmatic medical journals, as a therapeutic discovery of the first importance. Within a few days of beginning a daily course of injections, a reduction in stiffness and lessening of pain occurs, and there is improvement in muscular function, appetite and weight and patients have a sense of well-being. The symptoms unfortunately usually return in a few days if injections are stopped, so treatment may have to be maintained for life. This is a minor disadvantage compared with the problem of supply of cortisone, as at present it would cost a great deal of money to treat a single patient with it for a year. This costliness arises from the chemical nature of cortisone, which is 17-hydroxy-11-dehydrocorticosterone ('Compound E'), a sterolic substance which contains a complex ring system of carbon atoms which has not yet been synthesised completely in the laboratory. Many sterols occur naturally however, and one of them, desoxycholic acid, can be converted through thirty-six chemical stages into cortisone, and this is the present method of manufacture. Even the most efficient organic chemist can only expect a very small overall percentage yield after so many manipulations, and the starting material itself is not available in quantity, as it is prepared from ox-bile; this ox-bile comes from the slaughter house, and about 12,500 tons of beef cattle have to be handled to get enough bile to prepare sufficient cortisone to treat a single patient for a year. According to the latest figures published, production of the drug in America is about 200 grams a month, enough for the prolonged treatment of only about 60 patients.

If this promising material is to be brought into general use, new sources and methods of manufacture must be found. Clearly a much cheaper starting material is required: one which is potentially unlimited in amount, and preferably a material which is much closer chemically to cortisone than is desoxycholic acid in structure, so that the number of intermediate stages can be reduced. Already a most interesting and promising development has been announced, as it has been found that a compound called *sarmentocymarin* which occurs in the seeds of an African vine, *Strophanthus sarmentosus*, is chemically related to cortisone and can be converted into it in about 20 stages. This is still a lengthy and expensive transformation, but it promises to be a great improvement on the desoxycholic acid process, and while ox-bile is strictly limited in supply the vine seeds should be obtainable in any quantity. Plans are being considered for the large scale cultivation of the vine in the tropics, but any major project will have to be undertaken with caution, as the vine takes five years from planting before it bears seeds, and there is always the possibility that before the first crop is ready, some other more suitable starting material for cortisone manufacture may have been found; a compound similar to sarmentocymarin may be discovered in some other species of *Strophanthus* which is yet closer chemically to cortisone, or a suitable sterolic compound from some quite different animal or plant source may turn up. Within five years, cortisone itself may have been superseded by some other and more readily available drug. A great deal of intensive research is being carried out, and one would hope that history will repeat itself and that, like penicillin, once so rare and costly but now plentiful and cheap, cortisone may one day be the standard treatment for rheumatoid arthritis and related diseases and available for all who need it. At present, however, the afflicted must be patient, for a National Health or private prescription for cortisone could not be honoured.

Projection Television

To be able to project a large bright television picture on to a screen in the manner of a home ciné projector has long been the goal towards which television engineers have striven.

Over two years ago we published a description of the principles of the Schmidt optical system and its use to project an enlarged television picture (DISCOVERY, October 1947, p. 304). Visitors to this year's Radiolympia were able to witness the success of Philips' engineers in developing the technique and making commercially possible a television projection set capable of throwing a television picture on to a screen measuring 3 ft. by 4 ft. with a long

throw, or 16 in. by 12¾ in. with a short throw, as in a cabinet-type set.

The apparatus consists basically of two units, comprising a television receiver of orthodox type, and an optical unit in which is embodied a cathode ray tube on the face of which the picture first appears. This tube has a face diameter of only 2½ in. In fact it replaces the large tube which is used for direct viewing.

This tiny tube is the heart of the projection system. More than anything else it has made the big picture possible. This may be the more readily understood when it is realised that the tube is the only light source in the whole projector. It follows, therefore, that in order to allow for the inevitable losses involved in the magnification, reflection and correction of the image the first picture must be very bright. This brightness of the initial image is due to the very high-velocity bombardment of the fluorescent surface of the cathode ray tube created by a 25 kV electron beam. The tiny, brilliant picture measures only 1·4 in. by 1·86 in., roughly about the same size as a 35-mm. film.

The neck of the tube is very much thinner, proportionally to the larger sizes, than might be expected. This is because, in order to get the maximum deflection of the beam, the coils have to be mounted in much closer proximity to it. They themselves are small so that, with the tube, they may be packed tightly into the optical box to form a compact unit.

Having successfully produced a tube to give a picture of the requisite brightness, the engineers next turned their attention to the task of projecting it.

For reasons of cost any sort of lens arrangement was ruled out. Bright though the picture is, it would need to be many times brighter before anything comparable to the cinematograph projector system could be used. In the cinema, of course, the small picture (the film) is not, in itself, luminous. The brightness of the picture is here controlled by the wattage of the lamp behind the film, and a compromise between lamp power and lens aperture thus becomes possible. In the television projection system the brightness of the picture is constant. This means that the only variant possible is the optical system, and it was found that a lens arrangement would be altogether too bulky to make it a practical possibility, to say nothing of the cost entailed in producing lenses of the requisite size.

In the final result a folded version of the Schmidt optical principle was decided upon. The principles of this system have already been described in detail in DISCOVERY (October 1947). Certain important modifications were necessary before projected television could be made commercially effective, and it is safe to say that without at least one of them, projection television would still be confined to the laboratory.

The Schmidt system in its simplest form consists of a spherical mirror and a corrector plate. The spherical mirror collects the rays of light and concentrates them into a convergent beam while the corrector plate, which is nothing more than a weak lens of special profile, compensates for the spherical aberration of the concave mirror. From it the light rays 'fan out' again to give the big picture.

The siting of the cathode ray tube in the optical box in relation to the spherical mirror demands extreme accuracy, and it was in this connexion that the first of the major snags in designing the unit for mass production was encountered. It was found that the dimensions of the deflection and focusing coils and the necessary alignment apparatus interfered with the light beams as they bounced back off the spherical mirror.

As no amount of juggling with sizes and siting would eliminate the trouble it was decided to fold the light rays once by changing their direction by 90° and thus avoid the obstructions. This was done by surrounding the head of the cathode ray tube with a 45° plane mirror and mounting the corrector plate in such a position that the light from the spherical mirror was transmitted through it after its direction had been changed (see Fig. 1). This arrangement was found to work very well, and further experiment ensured that servicing the unit presented no great obstacle to the television service engineer.

It will be seen from the diagram that to produce a picture 3 ft. by 4 ft. at a distance of 8 ft. from the screen (which, by the way, is an ordinary home ciné screen of good quality) the direction of light is changed once only.

To produce a picture on a ground glass screen in a

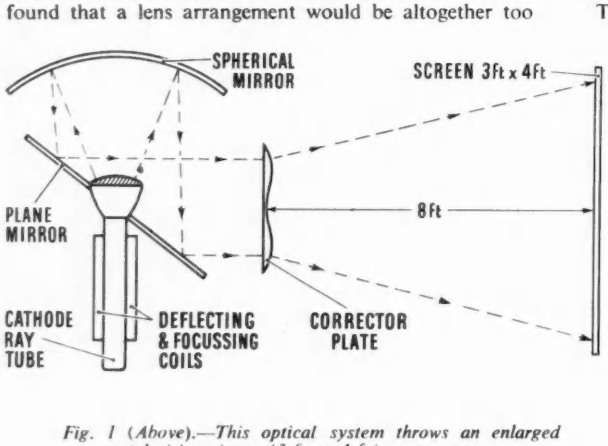


Fig. 1 (Above).—This optical system throws an enlarged television picture (3 ft. × 4 ft.) on to a screen.

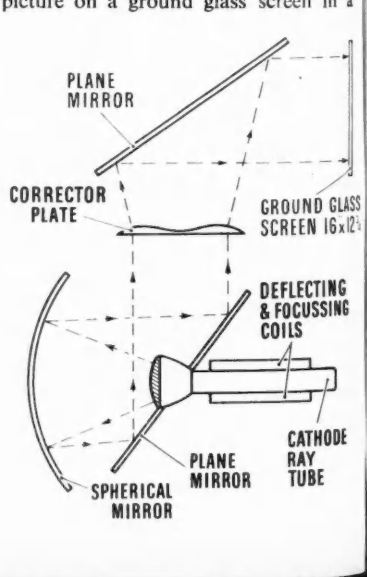


Fig. 2 (Right).—Modification of this system gives a smaller picture on a screen built into the receiver cabinet

cabinet by transmitted light (back projection), the correction plate must be changed for one of different profile. It is then merely necessary to lay the projection unit on its back inside the cabinet and, having sited it suitably, to introduce yet another 45° plane mirror in the path of the beam. This has the effect of folding the beam once more and forming a compact self-contained projection television cabinet receiver (see Fig. 2). Although it looks simple, the corrector plate gave more trouble from a production point of view than any of the other components of the optical system.

Owing to its special contours the labour involved in grinding it in glass not only made it too costly, but no method of mass-producing it could be found.

Gelatine was found to offer the best solution to the problem. Gelatine has a remarkable property that, suitably exploited, solved the lens problem. Refined to make it quite transparent, a 10% solution of it will, on being dried, shrink in one direction only to a tenth of its original size *exactly*. Thus by making a mould of a corrector plate exaggerated ten times and pouring the solution into it, one gets a perfect cast of the plate when the drying process is complete.

The question now is: Is this method of projecting television pictures the final one?

No one can say, because although one or two alternative systems are being talked about they have a long way to go before they reach practicality. The most promising are those that seem to solve the problem of the restricted amount of light available. These use material that changes its *opacity* under bombardment by the electron beam and they would thus appear to be able to make use of an independent light source after the manner of the cinema. It is understood, however, that other problems, mechanical as well as electronic, present themselves in quite formidable dimensions.

Trees of the Californian Gold Rush

DURING the past year California has celebrated the centenary of the great gold rush of 'Forty-nine. Britain's gold situation was not conducive to enthusiasm about the anniversary (which was anyhow distant in both space and time) but there is an odd, little-remarked link between Californian gold and England's trees which merits notice. The opening of the Far West, largely by or for gold-seekers, led to the finding of great trees, and it was between the years 1845 and 1855 that Britain received the first specimens of several species which are now familiar and important features of our parks and forests.

The first redwoods (*Sequoia sempervirens*) came in 1846. A 364-ft. tree of this species is commonly said to be the tallest living tree in the world. In this country there are already specimens exceeding 150 ft. in height, and a 90-year-old grove in Montgomeryshire is the heaviest stand of timber of any kind anywhere in our island. The timber is of good quality. Far better known is the wellingtonia (*Sequoia gigantea*), famed as the Big Tree, for one Californian veteran (which is probably over 3000 years old) contains the largest volume of timber of any tree in the world. Introduced in 1853, the conical wellingtonia now flourishes in parks and gardens in all parts of England.

The year 1853 also brought the western red cedar (*Thuya plicata*) from further north on the Pacific coast. This excellent tree yields the familiar cedar shingles, but it is, of course, not a true cedar nor any near relation of the *Cedrus* family. It grows well in Great Britain and is planted in many places where larch would have been used 100 years ago. It has the advantage, compared with larch, of being shade-tolerant, which means it can be employed to underplant other and older trees such as oak.

The African vine, *Strophanthus sarmentosus*, the seeds of which it is hoped can be used to increase the supply of cortisone.

The western red cedar has rather fern-like foliage, certain forms of which can be confused with the foliage of the incense cedar (*Libocedrus decurrens*), another accession from the Pacific coast in the same year of 1853. The incense cedar is now a common park or garden tree, of a fastigiate habit, giving a vase-like form. The following year there came a third, even more famous scale-leaved or fern-foliaged tree—Lawson's cypress. Though used in this country chiefly for ornamental purposes, and for hedges, Lawson's cypress yields timber of excellent quality. It is one of the few timbers suitable for making battery separators.

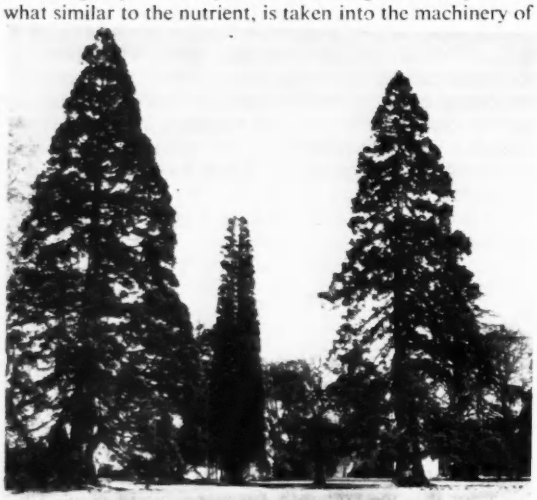
Looking back a little, one finds that the beautiful western hemlock (*Tsuga heterophylla*, which is rapidly gaining a reputation in British forestry) came in 1851. The great *Pseudotsuga taxifolia*, more familiar as Douglas fir, had arrived as early as 1827. Another tree connected with Douglas's name, the Monterey pine (*Pinus radiata*), was brought by Douglas himself from the Californian coast in 1833. The tree is not rare in England and indeed has some renown as the fastest-growing of all pines, but it is too frost-tender to be safe or happy save in the south-west. Most of those remarks are applicable also to the Monterey cypress, better known to most people as macrocarpa, for it is the botanist's *Cupressus macrocarpa*. This species (so much misused in Southern England as a hedge plant) came from the Monterey area in 1838. It is interesting that both the pine and the cypress had a narrowly restricted range and seemed quite likely to die out within a (biologically) short period of time, but both, when taken to other continents, greatly surpassed within 100 years the growth they had achieved in their native California.

Guanazolo

SOME of the research workers who are looking for substances which will control cancer are at present guided by the following line of thought. It is possible and even likely, they argue, that malignant tumours of various kinds handle nutrients received from the blood in a way that is somehow different from that employed by normal tissue. Associated with this different mode of using foodstuffs, tumour cells may perhaps require nutrients other than those needed by normal cells, or they may use the same materials in different proportions. If tumours do in fact differ from normal tissue in their nutritional needs, then it might be possible to interfere with the nutrition of tumours without impairing the well-being of healthy tissue.

It is not easy to see at first how a tumour served by the same blood supply as the rest of the body could be starved of some essential foodstuff without depriving normal cells too. In order to explore the possibility of doing this, we must turn to the great body of biological knowledge which has grown up in the process of discovering new substances that can cure microbial infections.

It happens that the best theory so far put forward to explain how the sulphonamides poison invading bacteria, without at the same time poisoning the human body, is based on the idea that the drugs interfere with bacterial nutrition. It is known that some bacteria require appreciable quantities of the nutrient substance *para*-aminobenzoic acid, while human cells do not. Some bacteria, in fact, use it to construct pteroylglutamic or folic acid, a vitamin which they need, as we do, and which we obtain ready-made in our diet. Sulphonamides are believed to interfere with the bacterial use of *para*-aminobenzoic acid in the following way. The sulphonamide, being chemically somewhat similar to the nutrient, is taken into the machinery of chemical transformations going on within the bacterium. Once in the machinery, the molecules of sulphonamide are incapable of undergoing the necessary transformations and fall down on the job, at the same time blocking the path for the molecules of the nutrient *para*-aminobenzoic acid, as refugees block the path of a defending army in a blitzkrieg.



Two specimens of wellingtonia (*Sequoia gigantea*), with an incense cedar growing between.

This work naturally suggested that the nutrition of tumours as well as bacteria might be blocked by vitamin antagonists. The first important efforts to synthesise antagonists to a vitamin and to use them against tumours were made with chemical analogues of folic or pteroylglutamic acid. Various small changes have been introduced into this molecule, and there is no doubt that the analogues made in these ways have an anti-folic acid action, and do inhibit tumours in experimental animals.

Unfortunately normal cells require folic acid, and these analogues interfere with their nutrition and are seriously toxic to healthy tissue. It is evident that a better starting point for this type of work would be provided by a substance which tumour cells require but which normal cells do not. Since tumours originate from normal human cells, and are in many respects very like them, it is clearly not easy to find such a substance. Is there any such nutrient; and, if so, can related compounds be synthesised which will block the use of this nutrient?

Until recently tumours were not known to require any substance which normal cells did not. A team of American biologists have, however, published a report which suggests that *guanine* is a substance of the type sought. In this report they describe experiments with 'guanazolo', the name they have given to a new substance, synthesised by other American workers, which is very like, but not identical with guanine. It appears from this report that guanine may be for the tumour cell what *para*-aminobenzoic acid is for the bacterium, while guanazolo may be the tumour's sulphonamide.

Guanine is a biologically important substance belonging to the class of purines; it is in fact amino-hydroxypurine. Guanazolo is amino-hydroxy, triazolo pyrimidine.

These workers have so far given guanazolo to various groups of mice suffering from three different kinds of malignant tumour. In each case, during administration of the new drug, the tumours, which were growing vigorously before, ceased to grow. On ceasing to give the drug, the tumours grew vigorously again.

It must not be thought from this that guanazolo will necessarily prove to be a satisfactory treatment for one or more forms of cancer. Even, however, if guanazolo proves to have no practical value, the discovery of its tumour-inhibiting property due to its interference with the tumour's nutrition, if confirmed by other scientists, is extraordinarily heartening. It seems to point a way forward towards an ultimate possible goal—the chemotherapeutic control of cancer.

REFERENCE

Kidder, G. W., Dewey, V. C., Parks, R. E. and Woodside, G. L. 1949: "Purine Metabolism in *Tetrahymena* and Its Relation to Malignant Cells in Mice." *Science*, **109**. 511.

The Scientific Evidence for Extra-Sensory Perception

S. G. SOAL, M.A., D.Sc.

At the end of his Presidential Address to Section D—Zoology of the British Association, Prof. A. C. Hardy made an announcement as startling as it was unexpected. He spoke as follows:

There is another matter which I feel it only right to mention if one is not to be intellectually dishonest. There has appeared over the horizon something which many of us do not like to look at. If it is pointed out to us we say: "No, it can't be there, our doctrines say it is impossible." I refer to telepathy—the communication of one mind with another by means other than by the ordinary senses. I believe that no one, who examines the evidence with an unbiased mind, can reject it; particularly the evidence of the vast number of experiments of the past twenty years and of that group of them which has passed successfully the statistical tests regarded as decisive when applied to normal scientific technique.

Professor Hardy then went on to suggest that perhaps our ideas on evolution may be altered if something akin to telepathy—unconscious no doubt—was found to be a factor in moulding the patterns of behaviour among members of a species. He postulated a group behaviour plan common to the members of a race which operated through the linking of individuals by the agency of telepathy. In passing it is worth noting that the late Whately Carington in his book *Telepathy* advanced a similar concept of a 'group mind' belonging to a species. Carington, for instance, thought there was more in the web-spinning activity of a spider than a merely inherited pattern of nervous behaviour mechanically coming into play in the presence of certain stimuli. He pointed out that the creature often had to adapt its behaviour to new situations and that this was accomplished by the individual drawing by telepathy on the stored up reserves of web-spinning experience which existed in the group mind of the species.

To what extent such ideas are likely to be accorded serious consideration by biologists I am in no position to say. My chief concern is with Professor Hardy's statement that in his belief no unbiased person who examines the evidence for telepathy can reject it. It is perhaps a little unfortunate that this announcement appeared at a time when the public mind was excited and mystified by the 'vaudeville' performances of two young Australian broadcasters, the Piddingtons. Many might suppose quite erroneously that the professor had been deceived or unduly influenced by these acts of pseudo-telepathy. I have, however, Professor Hardy's assurance that when he delivered his address he knew little or nothing of the Piddingtons, having just returned from abroad. His pronouncement in favour of telepathy was indeed based on a careful study over many years of the statistical evidence published in England and America.

Of the Piddington performance itself I need only say that any even moderately skilled illusionist could successfully imitate it with ease. I myself was asked by the B.B.C. to be one of the 'judges' but I pointed out that it was not my intention to be fooled by any fake performance and that I should insist on such conditions of control as would render fraud impossible. The B.B.C. withdrew their offer and thus relieved me from an embarrassing situation. I witnessed the performance, however, as a private spectator and formed a pretty clear notion of how most of the tricks were done. The repercussions of this affair are more serious than appears at first sight. On the one hand a large number of credulous people including even ordinary scientists and psychologists—merely because they could not imagine how the alleged 'thought-reading' was done—assumed illogically that genuine telepathy must be the explanation. A false and misleading impression was thus created to the effect that the art of telepathy had now reached such a stage of development that the faculty could be demonstrated to any audience with a degree of success between 80 and 100% even in cases where the odds against chance coincidence were hundreds or thousands to one.

The absurdity of this is manifest when it is recalled that generally speaking even the best subjects hitherto examined in this country and America show intrinsic rates of scoring which seldom exceed 10 or 12% when working on a one-in-five chance.

On the other hand, sceptical scientists may argue that the serious experimenters in telepathy obtain their positive results by using methods similar to those employed by the Piddingtons. It might for instance be suggested that small groups of young people eager to win cheap Ph.D. degrees conspire among themselves to delude influential witnesses into believing that they are seeing a fraud-proof demonstration of telepathy by card-guessing. I should hasten to add, however, that this misanthropic view of human nature—of academic human nature at any rate—is not altogether supported by an unbiased examination of those reports which claim positive results. The majority of card-guessing experiments published, say, in the Duke University *Journal of Parapsychology* furnish odds in favour of the hypothesis of extra-sensory perception which amount to no more than a few hundred to one. One feels that such results are not likely to achieve much newspaper publicity and that if the experimenters had wished to cheat they would have made a more conclusive job of it. It is true, however, that a few workers in this field (of whom the present writer is one) by employing specially gifted subjects have obtained over-all odds of an astronomical order. But these are exceptional cases each of which must be judged on its own merits.

It might be maintained that the present writer, admittedly a very successful investigator, was not a proper person to write a critical survey of the subject. But against this it must be urged that for many years prior to 1940 he was considered to be one of the chief critics and opponents of the card-guessing claims. For over ten years he worked on statistical investigations into telepathy and clairvoyance with scarcely a gleam of success. He can at least claim to know as much as any person living about the numerous sources of experimental error that are associated with such researches.

It is well-known that during its sixty odd years of existence the Society for Psychical Research has investigated a vast number of spontaneous cases which appear to support

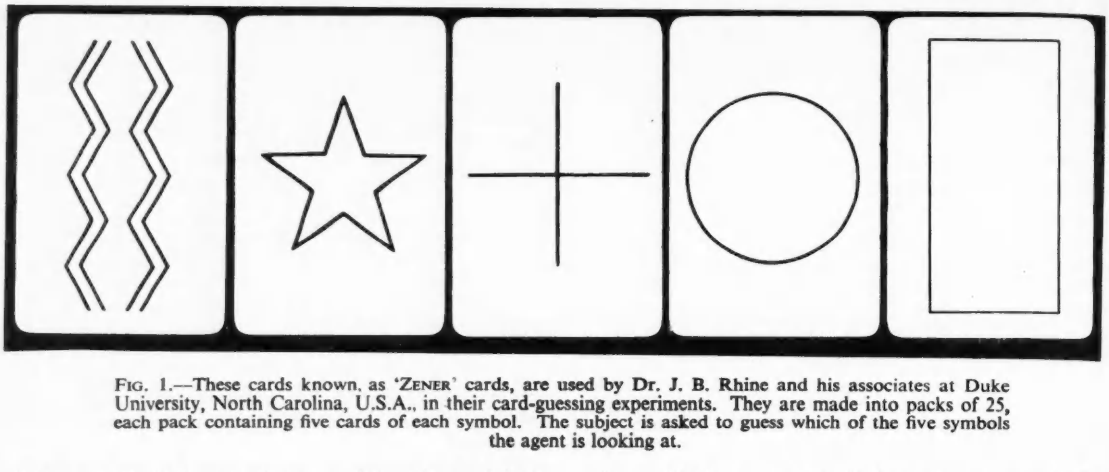


FIG. 1.—These cards known as 'ZENER' cards, are used by Dr. J. B. Rhine and his associates at Duke University, North Carolina, U.S.A., in their card-guessing experiments. They are made into packs of 25, each pack containing five cards of each symbol. The subject is asked to guess which of the five symbols the agent is looking at.

the hypothesis of telepathy. In many of these the percipient has a strong impression of some accident or calamity which has happened to a distant friend or relative. The impression may sometimes be so strong as to produce a visual or tactual hallucination so that the subject seems to see his distant friend clearly localised a few feet away from him and bearing on his person marks of the accident which has befallen him. Interesting as such cases are, their recital has done very little to convince scientific men of the reality of telepathy. Often one feels on reading such reports that the cross-examination of witnesses has been too superficial so that facts may have remained hidden which had they come to light might have indicated a purely normal interpretation. Again, much of this evidence suffers from the fact that often it is almost impossible to define the scope of chance coincidence in estimating its value. This is especially true in the case of the precognitive dreams studied by Mr. Dunne and others.

We shall, therefore, take no account of this class of evidence but confine our attention to the attempts which have been made to demonstrate telepathy by strictly experimental methods. Further we may omit all reference to the early experiments of pioneers such as F. W. H. Myers, Sir William Barrett, Sir Oliver Lodge and Mrs. Henry Sidgwick, who worked at a time when statistical method was still in its infancy.

The earliest statistical experiments of any importance were those of Dr. J. E. Coover a sceptical psychologist of Leland Stanford University who during the years of the first World War carried out experiments with playing-cards. Coover tested 100 students and collected 10,000 guesses. Before he began Coover declared that he would require odds of 50,000 to 1 against chance to be convinced of telepathy. His results actually showed odds of 200 to 1 against chance. This fact, however, he carefully ignored and it was left for Dr. R. H. Thouless and others to point out years later that had Coover persevered and maintained the same rate of scoring for another 15,000 guesses he would actually have achieved his desired odds. It is, however, quite possible that the original excess of correct hits was due simply to inefficient shuffling of the packs of cards. Coover's failure to continue with the tests remains a mystery of psychical research.

It was felt by many that playing-cards were unsuitable material for testing telepathy. For one thing there are too many symbols to bear in mind and some of these are too much alike. Again if a person chosen at random is asked to guess a playing-card he is more likely to choose an Ace than a card of any other denomination. Such habits of association might easily militate against the emergence of any genuine telepathic impression.

The Work at Duke University

Bearing this in mind Dr. J. B. Rhine a psychologist of Duke University, North Carolina, in about the year 1931 made the innovation of substituting for playing-cards other cards inscribed with five distinct types of geometrical diagrams—a circle, a square, a star, a plus sign and 'wavy lines'. The cards were made up into packs of twenty-five and each pack contained exactly five cards of each kind of symbol. In the early tests Dr. Rhine and the student to be tested sat on opposite sides of a table.

Dr. Rhine shuffled the pack and laid it face downwards on the table. He then asked the student to name the top card of the pack. Rhine recorded the guess, lifted off the card without letting the guesser see its face, and laid it face downwards on the table. The subject was then asked to guess the new top card and so on. When the twenty-five cards had all been removed one by one and placed in a pile without disturbing the original order of the pack, Rhine checked the cards against the student's list of guesses. The most likely number of correct hits to be expected if nothing but chance is at work is clearly five. If the student guesses through N packs this expectation is 5 N. The difference between the *actual* number of correct hits and the expected number is called the 'deviation'.

In order to estimate how 'significant' this deviation is, it is compared with a number called the 'standard deviation'. Now Rhine worked out his standard deviation by what is called the Binomial Formula. If N is the number of packs of twenty-five that are guessed then the standard deviation is $2\sqrt{N}$.

Thus suppose that the student guessed through the pack sixteen times (the cards being reshuffled before each run through) and suppose he obtained 112 correct hits. His

expectation is $5 \times 16 = 80$ and his deviation is $112 - 80 = 32$. The standard deviation is $2\sqrt{16} = 2 \times 4 = 8$.

We thus find that the *actual* deviation is exactly four times the standard deviation and we find from the 'normal tables' that such a deviation (either above or below the mean) would occur only once in about ten thousand such experiments by chance alone. There is good reason, therefore, for thinking that in the case of our imaginary student some factor other than chance was helping him in his guessing. But it does not follow of course that the result was due to any kind of 'extra-sensory' perception or E.S.P. as it is called. It might be, for instance, that the guesser learned subconsciously to recognise the figures on the faces by associating them with small specks or discolorations on their backs. This would be particularly likely to happen if the experimenter used the same pack over and over again. Or it might be (as was the case with some of Dr. Rhine's cards) that the cards were so badly printed that when held at a certain angle to the light impressions of the symbol showed through the back. When these sources of error were pointed out to Rhine he answered by reporting many successful tests in which the guesser was separated from the experimenter and the cards by a wooden screen.

There was one crucial series of 'clairvoyance' experiments in which the experimenter Dr. Pratt and the subject Hubert Pearce were situated in different buildings of Duke University. Experimenter and subject synchronised their watches before separating and Dr. Pratt lifted off the cards one by one from the pack at the rate of one per minute without looking at the symbols. Half a minute after Pratt had removed the card Pearce some hundreds of yards away wrote down his guess at the symbol on the face. There was no communication of any kind between the two buildings. At the end of each day's work both Pratt and Pearce handed in to Dr. Rhine sealed envelopes containing the lists of card symbols and the lists of guesses respectively, this being done before the pair had any opportunity to meet.

In all, Pearce made 1850 guesses and scored 549 hits as compared with the 370 which chance would predict. The odds against obtaining such an excess are of the order 10^{+9} to 1.

In this experiment it would appear that all sensory cues were completely ruled out and unless we postulate collusion between Pearce, Pratt and Rhine it is difficult to avoid the deduction that some sort of E.S.P. has been demonstrated.

Technically Dr. Rhine was in error in working out his 'standard deviation' by the 'Binomial' formula $2\sqrt{N}$ since his packs contained exactly five of each symbol and therefore did not constitute a random distribution. It would have been safer to have used $2{\cdot}04\sqrt{N}$ but the difference is so small as not to affect materially any of Rhine's conclusions. It may be added that a galaxy of statisticians have examined the card-guessing methods from every angle and no serious flaws have been found in them.

If on the experimental side Rhine's earlier methods were open to objection on certain counts, that is certainly not the case today. He and his disciples have since 1938 tightened up their techniques to an amazing degree. Those psychologists like Professor Chester Kellog who predicted that as the experimental conditions were improved the scores would fall to chance level have had the shock of their lives.

Routine methods were introduced into the parapsychological laboratory which made errors in recording results and checking scores practically non-existent. In the telepathic tests agent and guesser were placed in two separate rooms with usually a third room intervening. Two or more investigators have been present at practically all experiments and it has been made impossible for one experimenter to cheat without the knowledge of the others.

In spite of all these multiple precautions the scores have remained significantly above chance expectation.

There have been many successful repetitions of Rhine's work in America, some of them under exceptionally good conditions of control. To mention only one there was the famous experiment conducted by the psychologist Dr. Lucien Warner at Greenfield, Massachusetts, on October 8, 1937.

The subject M. H. was locked in a room on the first floor. Dr. Warner and his colleague Mrs. Raible were in a room on the second floor which was not directly over the percipient's room. The doors of the two rooms were so placed that anyone standing near the door of one room could not possibly see the door of the other. By means of a one-way signalling apparatus the percipient was able to indicate to the experimenters when she had recorded her guess by simply pressing a key which lit a lamp in the room above. At each guess the pack was shuffled and cut and a card drawn out. In order to obviate the possibility that the hearing of M. H. might be sufficiently acute to enable her to identify the card selected from the sounds made by the experimenter's pen as he wrote down the symbol, the symbol on the card face was not recorded until after the percipient had written down her guess. After each guess the card was replaced in the pack which was then reshuffled and the next card drawn. This would ensure a binomial (or random) distribution. It was decided to do exactly 250 guesses before the experiment started. Instead of the fifty correct hits chance might be expected to produce M. H. scored ninety-three which corresponds to odds of the order 10^{10} to 1.

The Discovery of Displacement

In England good subjects seem to be rather difficult to find but three of outstanding gifts have been studied intensively during the past ten years. These are Miss G. M. Johnson tested by Mr. G. N. M. Tyrrell by means of an ingenious electrical machine, Mr. Basil Shackleton who scored positive results over a period of two years in experiments conducted by Mrs. K. M. Goldney and myself and Mrs. Gloria Stewart with whom Mr. F. Bateman and I have been experimenting for the past four years.

The late Mr. Whately Carington working in conjunction with Professor C. D. Broad, Dr. R. H. Thouless and the statistician Dr. Irving invented a new technique for the study of extra-sensory perception which incidentally led to the discovery of a phenomenon called 'displacement'.

Mr. Carington pinned up in his heavily curtained study at Cambridge a drawing of a fairly simple object chosen by opening Webster's dictionary at a random page and selecting the first object on the page which could be reasonably drawn. The sketch remained on view from 7.0 p.m. until 9.30 a.m. on the following morning. A number of persons of no special psychic ability living in

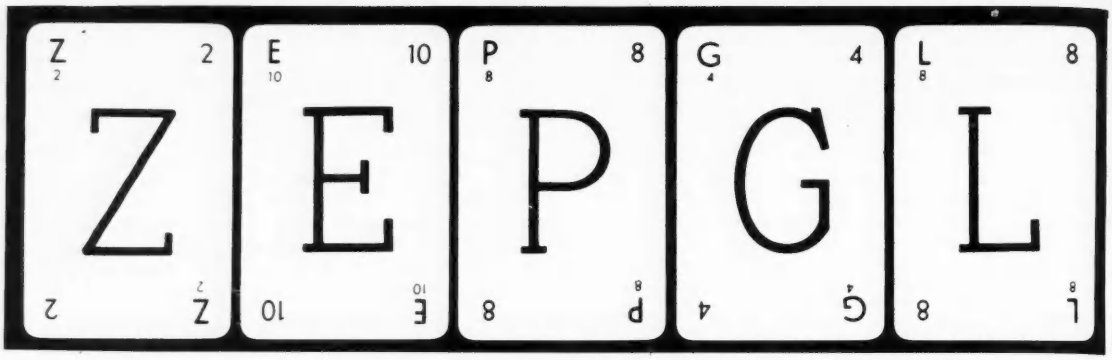


FIG. 2.—Lexicon cards used by the writer in his experiments with the subjects Mr. Basil Shackleton and Mrs. Gloria Setwart. The letters are the initial letters of the names of five animals: P = PELICAN, E = ELEPHANT, G = GIRAFFE, Z = ZEBRA, L = LION.

this country or abroad were asked to draw their impressions of the object exposed in Carington's study. The drawings which were done in special books provided for the purpose were posted to one of the experimenters. The tests done on ten consecutive days constituted an experiment and there were five such experiments in all separated by intervals of a few days. The several hundred drawings sent in were randomised and an identifying code number was affixed to each drawing. The drawings, together with the fifty originals, were forwarded to Mr. M. T. Hindson, an independent judge, who had no normal means of discovering on which evening or in which of the five experiments any particular sketch was made. Mr. Hindson was asked to match each drawing against each original; if he thought the drawing clearly resembled the original he was to assign to it a score of one, if there was a doubtful resemblance a score of one-half and if he judged there was no resemblance a score of nought was assigned.

Making use of a statistical formula due to W. L. Stevens, Carington was able to show that there was no significant tendency for the percipients to score above chance on the original shown on the evening on which their drawings were made. There was, however, a highly significant tendency for the drawings sent in during one ten-day experiment to resemble the originals used in that experiment more than they resembled the originals of the other four experiments. In other words if, say, SHIP, HAMMER, TREE were originals selected by Carington for the second experiment, there would be proportionately more ships, hammers and trees sent in during the ten days of the second experiment than during the ten days of any other experiment. If, say, the original SHIP was exhibited on a Wednesday the percipients would tend to draw ships not only on Wednesday evening but also on Tuesday and Thursday evenings. That is to say there was a marked tendency to produce ships even twenty-four hours before Carington chose SHIP at random from the dictionary!

This was obviously a discovery of first-class interest. The further one receded backwards or forwards in time from Wednesday evening the less likely were the subjects to draw ships. Carington's results showed both a retrocognitive and a precognitive effect and of these the precognitive was slightly the more marked. It was as if a man with a rifle standing in front of a row of targets sometimes hit the target he was aiming at but sometimes that on the immediate left or right of it. Only in our case the row of targets were not in space but in time.

Now at the time when Carington made this observation I had just completed a long series of card-guessing trials in which I had given tests to 160 persons and collected 128,350 guesses. Neither in the tests for telepathy nor for clairvoyance had I obtained any success. It was at this stage in the autumn of 1939 that Carington urged me to re-examine my telepathy results to see if any of the subjects had been scoring hits not on the card the agent was looking at but on the card one ahead or one behind in the sequence. To my astonishment I discovered two persons whose records displayed the very effect Carington had anticipated. Mr. Basil Shackleton and Mrs. Gloria Stewart had been scoring significantly on the cards immediately preceding and immediately following the target card.

I cannot continue here the story of how Mrs. Goldney and I sought out Shackleton and persuaded him to submit to a long series of further tests under the most rigid conditions of witnessing and control and of how we discovered that the displacement of his guesses was in all probability a displacement into future or past time. But the reader who is unable to obtain our original report (*Proceedings of the Society for Psychical Research*, December 1943), will find a short popular account of this research in Penguin *Science News*, No. 9.

It had long been suspected that telepathy was unaffected by space. That it should be to some extent independent of time would perhaps appear to follow from the fact that in the four dimensional universe of Minkowski and Einstein time and space are indissolubly welded.

I might add that Sir Arthur Eddington was very intrigued by these Shackleton results, and shortly before his death declared that they fitted in with his own ideas of personal time.

Is Telepathy Established?

I shall now venture to give my own answer to the question which arises out of the remarks of Professor Hardy: "Is there scientific evidence for telepathy which no one who examines it can reject?"

I would reply: In a sense yes. Unless one is prepared to

postulate wholesale fraud among a considerable number of persons of academic standing, the recorded evidence of the best controlled experiments cannot be refuted.

But telepathy is not established in the sense that a fact in physics or chemistry is established. If dilute hydrochloric acid is poured on chalk we know that an invisible gas is set free which extinguishes a lighted taper and turns lime water milky. Anyone can check up this fact for himself at any hour of the day or night. But that is not all. We understand how to produce the gas in numerous other ways and indeed the whole theory of this simple reaction and its exact relation to the other facts of chemistry is well understood.

Now an experiment in telepathy can sometimes be successfully repeated but the repetition is always an exceedingly difficult and chancy affair. A telepathic subject who is one day producing extra-chance scores against odds of 100,000 to 1 may in a fortnight's time be producing only chance results.

The trouble with telepathy is that so far it does not seem to link up with any other known facts of science. It is an unfriendly intruder that has got into the scientific realm from heaven knows where. Its comings and goings seem impossible to control. By dint of assiduous questioning and experiment we have found out a few isolated facts about it, but these facts are of the kind that give us very little notion of the states of body and mind which favour its appearance. Two workers in America, Dr. Betty Humphrey and Dr. Gertrude Schmeidler, are trying to correlate personality types with telepathic ability by making use of such standard psychological tests as the Rorschach test. Some of this work seems promising but progress is inevitably slow. Needless to say that once we discover how to induce telepathic percipience in quite normal persons, possibly by the use of some drug, we shall go ahead.

The Attitude of Science

In the meantime how is Science to behave towards this strange and unwelcome visitor? The ordinary scientific worker—the chemist, the physicist, the engineer—may perhaps justly plead that it is not his job to entertain her. But the psychologist is in a more embarrassing position for she is a distant blood relation, whom for certain reasons he does not consider to be quite respectable.

Psychology has for a hundred years been trying to apply to the study of the mind the mechanistic methods which have proved so fruitful in physics and chemistry, but with little success. The late Professor McDougall confessed that after thirty years he felt he had learned practically nothing about the mind. The fundamental problem of the mind-matter relation is still unsolved. Physiologists may be able to trace certain electrical or chemical changes in the nerve fibres leading to the cerebral cortex when light of a certain wavelength impinges on the retina but no one has been able to explain how or why these result in what human consciousness knows as a *sensation of* (say) *green*. No one knows how we are able to conjure up a mental picture of a past event that is no longer being perceived. On the *mechanistic* theory of life and mind the phenomena of perception and memory involve insuperable difficulties. We accept them unthinkingly only because we have been familiar with them from our infancy. Had we been equally familiar with telepathic interaction between minds we should no doubt have accepted it without inquiring what place it has in the scientific scheme. Only the philosophers would have argued about it.

But if we agree with Eddington that the fundamental stuff of the material universe is in the last analysis mind-stuff and that brains are just islands of this mind-stuff which have achieved self-consciousness, we shall have less difficulty in admitting telepathy into the universal scheme of things.

READING LIST

The following four books are recommended to those who wish to read further into the subject.

Extra-Sensory Perception after Sixty Years. By J. B. Rhine, J. G. Pratt, C. E. Stuart, B. M. Smith with J. A. Greenwood. (New York, Henry Holt & Co., 1940.)

The Personality of Man. By G. N. M. Tyrrell. (Pelican Book, 1946, published by *Penguin Books*.)

Telepathy. By Whately Carington. (Methuen, London, 1945.)

Experiments in Telepathy. By René Warcollier. (Allen & Unwin, London, 1930.)

Among the most significant papers dealing with the subject are the following:

"Experiments in the paranormal cognition of drawings." By Whately Carington, Part I, *Proceedings, Society for Psychical Research*, 1946, **40**, 34–151.

"Experiments in Precognitive Telepathy." By S. G. Soal and K. M. Goldney. *Proceedings, Society for Psychical Research*, 1943, **47**, 21–150.

"Studies in Extra-Sensory Perception: A Review of all University of Colorado Experiments." By D. R. Martin and F. P. Stribic. *Journal of Parapsychology* (Dec. 1940), **4**, 159–248. (Duke University Press, U.S.A.)

"A Test Case." By Lucien Warner, *Journal of Parapsychology*, 1937, **1**, 234–238.

Rodent Ecology

CURRENT research at the Johns Hopkins School of Hygiene and Public Health at Baltimore, U.S.A. into the control of rats has revealed interesting features of rat behaviour. Of the three factors which limit populations, environment, predation and competition, the latter has been found the most effective among rats. Where improvement in sanitary conditions is limited to a reduction in the food and junk which is left about, the rat population declines only about 50%; where housing is drastically rehabilitated, the rats may decline about 75%. Killing methods by traps, cats, bacteria, poisons and so on are still under trial but so far it has been found that killing merely makes room for more rats to grow up and actually increases the yield of rat flesh per acre states the 1948 annual report of the International Health Division of the Rockefeller Foundation.

The competitive factor is largely responsible for keeping the numbers of rats within the capacity of an environment. Like many other vertebrates, rats develop a social hierarchy, and by means of fights they determine their place in the social scale. High-ranking individuals have ample food and reproduce regularly, whereas the reverse is true of low-ranking individuals.

Rats which grow up together are much more tolerant of one another than rats which first meet as adults, and young rats soon learn their rank. After weaning they will crowd among older rats with complete indifference and may even attempt to remove a morsel of food from an adult. At first the adult ignores the juvenile but soon begins to fend it off with fore or hind feet. If the young rat continues to interfere, the adult rolls it over on its back and gives it a severe kicking. Among young which belong to a family group social status is reached through a process of play fighting in which marked antagonism is absent: serious fighting among such individuals is rare even after they grow up.

The First International Polar Expedition

FRANK ILLINGWORTH

THE first international polar expedition, the Anglo-Scandinavian Expedition to Queen Maud Land in the Norwegian-claimed sector of the Antarctic at about longitude 0° has now set out. The expedition-ship sailed in November, and the shore party, comprising fourteen scientists and technicians from the three participating countries, will remain in Queen Maud Land until February 1951.

The expedition's equipment is as excellent as its scientific programme is wide. Indeed, the wintering party will probably be richer in equipment and scientific personnel than any in the annals of polar exploration. This has been made possible by the painstaking work of the British field-parties maintained by the Colonial Office in Graham Land since 1943, and by the pooling of gear, personnel and knowledge arising from the labours of polar specialists in London, Oslo and Stockholm.

Co-operation on an international basis has made available a greater range of equipment than would have been the case in a purely national venture. For example, Norway is providing reindeer pelt for clothing, sledges, and ski-equipment, in which she excels. The expedition's scientific instruments are being purchased in Britain and Sweden. All three countries are combining in the provision of food, with Britain supplying the pemmican (sledging rations) following extensive experiments by British parties in Graham Land.

The Graham Land bases offer ideal field conditions for the development of polar equipment, and there is no doubt that Britain is well ahead of other nations in this sphere of research.

British polar tents are the finest available. Light, strong, pliable and easy to erect, they are proof against the fantastic 120 m.p.h. blizzards of the Antarctic. Tested on the annual 900-miles sledge journeys from Marguerite Bay and the other Graham Land bases they were used by the recent U.S. expedition led by Commander Finne Ronne in preference to American polar tents.

Another product of British experiments is a new form of pemmican, which is a mixture of powdered lean meat and melted fat, hardened into bricks sufficient to sustain a man for twenty-four hours. Generations of explorers have cursed pemmican for its uninteresting taste. The men of the Anglo-Scandinavian Expedition will have the benefit of pemmican flavoured with beef and mutton.

The wide range of equipment provided by this country is a credit to the painstaking research of British industry and to the enthusiasm of the British explorers who have designed and tested it under extreme conditions.

The generating plant which will power the radio at the expedition's base hut, and the radio equipment itself, will be provided by this country. The crates of woollen clothes to be loaded into the expedition-ship in November have been made by one of the firms that equipped Scott's last expedition, and plastic sledge runners are the result of field tests in Graham Land. These sledging surfaces are said to last longer than steel and do not 'clog' with snow thereby adding to the weight of the sledge. They may be used in preference to Norwegian sledges when the wintering party puts ashore.

Plans for the expedition were drawn up in consultation between the Royal Geographical Society, the Scott Polar Institute, Cambridge, the Norwegian Polar Institute, Oslo, and in Stockholm, under the general direction of Dr. Harald Sverdrup, who accompanied Amundsen.

Scientific and technical personnel have been selected from the three countries. Norway is to be responsible for the meteorology and surveying, Sweden for glaciology, Britain for the expedition's geological work. The British members of the wintering-party are Mr. W. M. Smithenbank, an Oxford glaciologist, who will work under the direction of the noted Swedish glaciologist, Professor Hans Ahlmann; Mr. Alan Reece, geologist (Imperial College); Mr. G. de C. Robin, a physicist from Birmingham University; and Mr. E. F. Roots, a Canadian geologist. A Royal Air Force unit, under the leadership of Squadron-Leader Walford, will spend the first summer in Queen Maud Land, returning to Britain when the expedition-ship pulls out next February at the beginning of the Antarctic winter. The wintering-party will be led by the Norwegian, Captain John Giaever, whose experience includes twelve winters spent in the polar regions.

It should be stressed that the undertaking contains several unknown factors. We know little about Queen Maud Land, very little. Clad in ice and snow, and whipped by ferocious blizzards, it lies behind a barrier of sea-ice in many places beyond penetration, a south polar enigma that may hold the key to a scientific problem of particular interest—is the world growing warmer?

The venture was conceived by Professor Hans Ahlmann, the Swedish explorer and glaciologist, following the publication of the German report on the expedition sponsored by Field-Marshal Goering. This Nazi venture to Queen Maud Land (1938 39) discovered large snow-free areas in the midst of the vast inland ice. Situated at the feet of high, unexplored mountains, the Germans took aerial photographs of these oases from the air, threw down iron bars stamped with the Nazi swastika, and returned to Germany in time for the war.

Retreat of the Ice

The German discovery suggested that the weather in the Antarctic is growing steadily less harsh, as is the case in the Arctic. There is ample evidence that the climate of the Northern Hemisphere is growing less cold. For example, charts drawn from aerial photographs indicate that the twenty years since 1924 have seen drift ice to the north of Russia diminish by some one million square kilometres. The early explorers found the North-East Passage jammed with ice beyond penetration, yet today convoys of Soviet merchant ships, helped by ice-reconnaissance aircraft and ice-breakers, pass through these same waters every summer to collect the produce of forest and tundra. In Iceland, melting glaciers have exposed land last cultivated five centuries ago. Greenland's ice cap appears to be receding,

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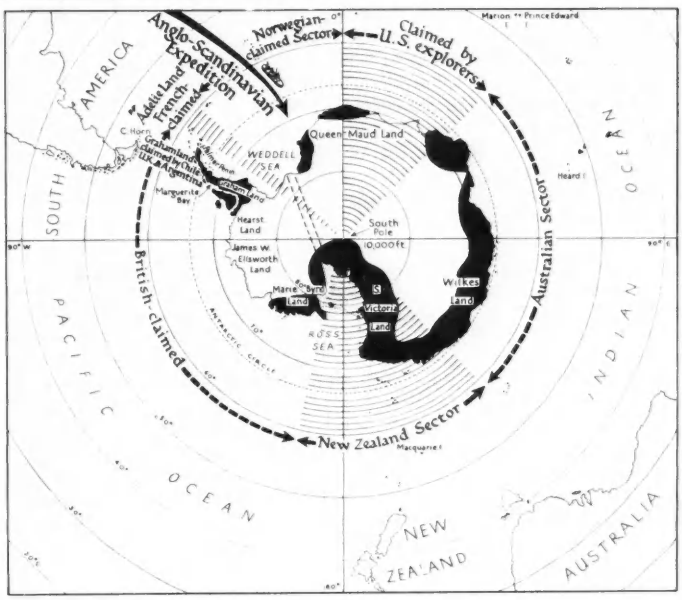
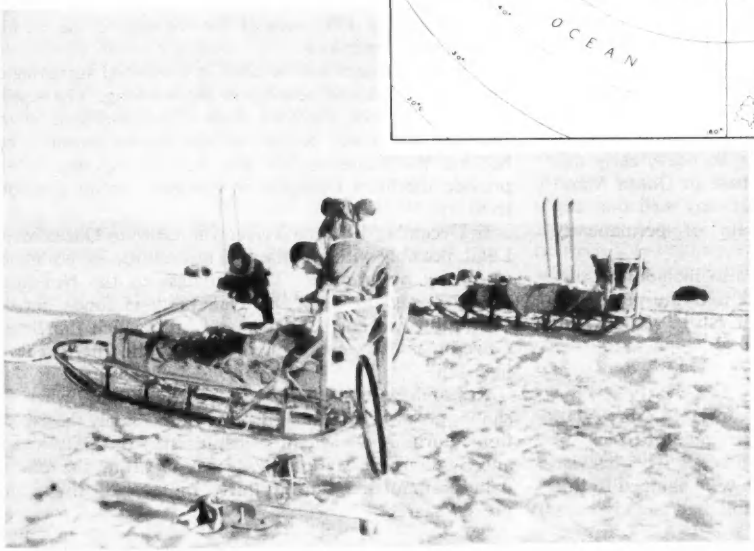


(*Top*)—Huskies, here pictured hauling a sledge in Graham land, will haul supplies on the Queen Maud Land Expedition. (*Bottom*).—British sledges being tested in the field. A new plastic sledge runner will will make sledge travel easier.



and there is evidence that the sea-ice to the north of Canada is less heavy than it was at the time Franklin died in search of the North-West Passage. Rabbits are breeding in Arctic areas where forty years ago they could not have survived; cod, once rare in Greenland waters, are now the Greenlander's staple diet. The long trips made by modern trawlers are not entirely due to over-fishing in coastal or 'home' waters: some fish, notably herring and halibut, are moving steadily northwards. Likewise the tree line in Scandinavia extends farther north than was the case fifty years ago. Even here in Britain, winters are generally less severe than they were a century ago, when the freezing of the Thames was nothing uncommon. The cause for this 'warming up' is not known.

Professor Ahlmann suggests that the ice-free areas of Queen Maud Land—one writer has called them, "temperate oases in a world of everlasting south polar ice"—could provide valuable data in the study of climatic history and ice recession.

Following the publication of the German report, Ahlmann approached the Royal Geographical Society with a plan for a joint British-Norwegian-Swedish expedition to investigate the oases in relation to world climatic fluctuations.

Parallel research is to be made into the effect of Antarctic weather on the inhabited areas of the Southern Hemisphere and on its communication routes.

The storms that rage across an ice continent exceeding six million square miles affect weather conditions over much of the South Atlantic, South Pacific and South Indian Oceans, and a chain of circum-south polar meteorological stations would make for safer air and sea communications over at least part of the Southern Hemisphere, possibly improved agriculture (one natural result of improved long-range meteorological forecasting) and certainly improved whaling.

Since the end of the war South Africa has set up meteorological stations in Marion Island and Prince Edward Island (in the Southern Ocean); Australia has established weather research stations in incredibly wild and blizzard-stricken Heard Island and McQuarrie Island; the Argentine and Chile maintain meteorological stations in Graham Land (due south of Cape Horn), and the Colonial Office has recently extended its chain of meteorological outposts in this, British, territory. The French would by now have had a meteorological station in Adèlie Land (French Antarctic) if the sea-ice had not been so heavy early this year. Meanwhile, a meteorological base in Queen Maud Land would be another link in what may well one day become a circum-south polar chain of permanently manned weather stations.

The expedition's programme will also include the surveying of new territories discovered, and the geological work for which the British contingent will be responsible.

Aerial Reconnaissance

The expedition-ship, the *Norsel*, a converted Norwegian sealer, called at a British port in November to pick up the British contribution towards success. The heavier equipment, and the sledge-dogs which were shipped to this country from Graham Land last spring, had already been taken 'south' through the blue-green pack ice on a whaler. The heavy equipment includes a two-ton apparatus designed to bore 600 ft. into the Antarctic ice cap; and to the thirty dogs from Graham Land are added thirty from Greenland. Dogs and gear will be transferred from the whaler to the *Norsel* in Antarctic waters.

The supply ship will carry two Auster aircraft, which will be used for reconnaissance work, both in helping the *Norsel* through the ice fields and in searching for a suitable location for the base camp.

We cannot be sure that the expedition will succeed in getting ashore. As previously stated, a French expedition to Adèlie Land was forced back by pack ice last February; and the Colonial Office's south polar supply ship, the *John Biscoe*, failed to relieve the scientific and survey outpost at Marguerite Bay, Graham Land.

But the *Norsel* has been reinforced for work in heavy ice, and all being well she will succeed in breaking through the floes and put the wintering party and its kit ashore.

The landing will be an anxious time. The Antarctic summer is brilliant and the 'labour squads' may work stripped to the waist so long as there is no wind. But a sudden blizzard brings a steep fall in temperature, and a change of wind can cause the dreaded pack ice to bear down on the coast. Thus no time must be lost in unloading. Day and night the 'labour squads' work with winch and pulley, rope and landing-rafts, to land the sectional-built base-huts, put the rations ashore, and the coal, petrol, scientific instruments. . . .

The first months in Queen Maud Land will be spent establishing a base camp. This will entail erecting base-huts from tongued-and-grooved joinery made in Sweden, packing a refrigerator chamber from the 100-ft.-thick ice shelf for the 20 tons of whalemeat for the huskies, building-in the generators, and completing the hundred-and-one other jobs required in establishing a home for fourteen men in a world of ice and snow.

When the base is established a line of food and petrol dépôts will be laid for the benefit of the coming sledge journeys across 400 miles of the ice cap to the ice-free oases in the hinterland.

The two Austers will be used in the initial surveying of the route inland, and possibly in dépôt-laying. The expedition's equipment includes three Canadian-built snow-tractors. These will be used on the shorter journeys, but because the weasel-tractor has limitations, huskies will provide the main transport in crevassed or mountainous territory.

In December 1950 the *Norsel* will return to Queen Maud Land, possibly with a Norseman aircraft for an ambitious air-survey programme. The purchase of the Norseman may depend largely on the availability of funds, for the expedition will cost £100,000, towards which the British Government has contributed £20,000 and the Swedes a like sum.

Research will continue until February 1951, when the shore-party will be taken off. And immediately the expedition returns home scientific institutes in Britain, Norway and Sweden will begin the vast task of sifting the mass of data accumulated during thirty months of continuous polar research.

Science, Efficiency and Industry

ARTHUR MARSH, M.A.

In an article in the November issue of DISCOVERY the writer discussed the economist's problems in attempting to measure industrial efficiency and outlined what had already been done in this field, particularly in making productivity comparisons between different countries, industries and firms. He concluded that such comparisons were the best available at the moment for the purpose of getting a broad picture of the situation, and that the gap between the amount the average American and the average British worker could produce before the war in an hour (about 2 : 1 in favour of the American) was so great that we could not afford to ignore it. He now discusses what steps can be taken to narrow the productivity gap.

ECONOMIC analysis of a situation is not useful unless it suggests causes and remedies. Why is American productivity superior to ours? Can we improve the situation? What part can the scientist play in improving it?

The proximate cause of the difference is plain for all to see. If we assume that the effort of labour is about the same on both sides of the Atlantic (and we have no reason to suppose otherwise), American industry is, on the whole, technologically superior to ours. The figures quoted in the November issue, and the diagram in this, seem to suggest that industry in both countries is rather like the curate's egg, but that in America the good part is larger. For every man at work the Americans have, on the average, more equipment than we do, and they use it better. The details of the picture may become clearer when more reports of the Anglo-American Productivity Council are published. Meanwhile we only have to refer to its recent publication on Steel Founding, to the Reid Report on Coal, the Platt Report on Cotton, the Bossom Report on Building, or, indeed, to any of the Working Party Reports, to see what the experts have already found. The Reid Report had no hesitation in attributing a large part of the difference in productivity to technical backwardness in this country; the Platt Report put first among its reasons for American superiority more up-to-date plant, more scientific method in the utilisation of labour, and higher operating efficiency; the report on Steel Founding, although it emphasised the psychology of 'productivity-consciousness' in the United States, came to very similar technical conclusions. Moreover, from the fact that it is possible to trace a wide productivity gap between the United Kingdom and the United States even back to the 1870s, it is clear that the Americans not only have more modern equipment and methods than we have, but have also kept them up to date over a period of time. They are more ready than we are to scrap plant, processes and methods and to set up new ones.

This being so, the economist's cure for the lower productivity of this country's industry is a large and continuing dose of investment; he advises that we ought to spend money on investment goods (*i.e.* machinery, equipment for new technical processes, and so on), pay more attention to the layout of plant and to management techniques, and that we ought to keep on doing this in the future so that our industry will always be up to date. At present there are economic difficulties in the way of putting some of this advice into practice. If we spend money in investment we cannot also spend it on things to eat and wear and put into our homes; if we overspend we may only succeed in making prices rise; and, finally, it is possible that the physical extent of re-equipment is limited at the present time by lack of investment goods. Those that are available are often made in dollar countries from which we cannot afford to buy more than we can help buying. Nevertheless, these difficulties do not exempt us from doing what we can to add to our investment goods, rearranging what we have (redeployment), and improving our methods of management; nor do they exempt us from accepting in principle the idea of getting our country's equipment up to date and keeping it up to date as soon as we are financially able to do so.

On this point the economist is in complete agreement with the scientist, though this agreement has seldom been commented on. It is the role of the scientist to widen our knowledge of the physical world, and to devise new products for the use of man. Most of these products have to be produced commercially before they are sufficiently cheap to reach the consuming public. Money has to be put into research, say on a new drug; then, the drug having been produced in the laboratory, more money has to be put into more research in order to produce a plant which will manufacture the drug economically; the plant has then to be bought, and finally the public gets the benefit of the original discovery when the drug appears in the shops. In short, before we get either our new drug or the new equipment for making it we have to do quite a lot of 'investment' in the economist's sense of the word, and, of course, we often raise our productivity in the process. New plant is likely to lead to more efficient production and more saving of manpower than old plant.

It is no accident, therefore, that while the economist has been complaining about the comparatively low productivity of this country, the scientist has been complaining that there has been a serious lag in the application of the findings of scientific research to industrial processes and techniques. The failure of much of industry to explore the commercial possibilities of academic research (of which, on the whole, there has been no serious lack, we are told), and to put these to practical use has its almost exact equivalent in the low level of industrial investment in some fields before the war and the relatively low level of production per man-hour which has resulted from this. If we can account for the discrepancies in productivity between this country and the United States we can presumably also account for the 'frustration of science'. We may be able to explain why industrialists have often been reluctant to adapt scientific developments to commercial use and to change their plants and methods of working to suit modern conditions.

Our first instinct is probably to blame the British business-man for his lack of initiative and foresight. This line of approach is not entirely without its justification. It is fairly plain that the American business-man has often been much quicker off the mark and much more imaginative than his fellow over here. There has been distinctly more 'science-consciousness' among American

manufacturers than among British. Much British industry still believes in 'common sense and hard work', and in rule-of-thumb methods as being the best way of achieving results. (So they do, of course, but in a very slow and roundabout way which a more scientific approach would do much to cut short.) Some contemporary British industrialists are very contemptuous of this attitude, but admit it to be widespread. At a conference on Industry and Research organised by the Federation of British Industries in 1946, one such business-man remarked: ". . . while hatred of science has given way to a mild disdain, or even to a good humoured tolerance, during the last fifty years, we have not yet, as a nation, become whole-hearted enthusiasts—in spite of the obvious success of many enterprises great and small who have long 'believed' in the value of science. There must be thousands of enterprises who think that they can carry on in the future as in the past, relying on common sense and ability." If this is so, it may be that only the advent of a new generation of business-men (it is often said that it is easier to talk science to sons than to fathers), will solve the difficulty, or the influx of new and more powerful technician/administrators on the lines which Mr. Burnham predicts in his book *The Managerial Revolution* (1941). Or perhaps the situation will be improved if there is a reversal of the tendency of the second and third generations of industrial leaders in this country to divert their energies towards establishing higher social, as distinct from better financial, positions.

Resistance to Technical Change

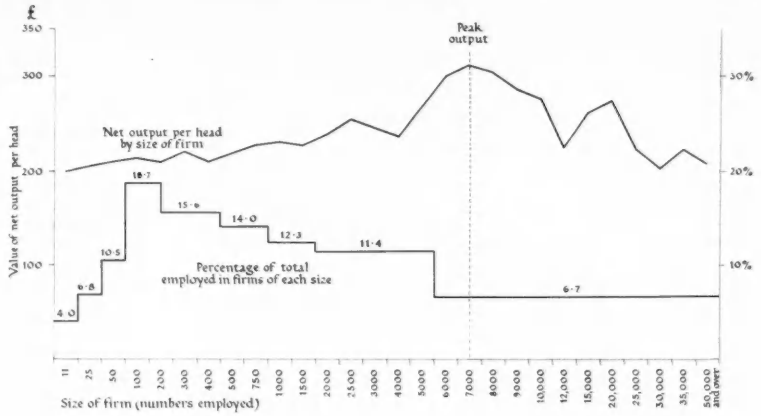
It is not sufficient, however, to leave the point here. It is clear that economic circumstances have not, for some reason or another, compelled the British industrialist to become science-minded. In the first place, it might be said, he has often had to meet the resistance of organised labour to technical change. In some cases this has been true, and the reasons for this attitude, which is much less marked in the United States, lie far back in Britain's industrial history, and are important enough to merit a separate article of their own. In the second place, he has not, on the whole, been faced with a situation in which labour was dearer to obtain (relatively) than investment goods. This was the situation in the United States during the early years of its industrial growth, and the tradition of employing a high ratio of capital to labour seems to have persisted. In this country labour has been relatively cheap, and when this is so there is a marked tendency to neglect equipment which also lasts longer and is more difficult to lay off at short notice without financial loss. Thirdly, it is true that in order to have a high productivity two circumstances have to be present—production usually has to take place on a fairly large scale, which entails large plants and mass-production methods; and there has to be a market large enough, and willing enough, to take standardised products in a large quantity before production is worth while. These circumstances have, it is argued, been present in the United States, but not in this country, and the British industrialist has been quite right in concentrating on specialised products which have been relatively more expensive but have provided the public with what it asked for. Fourthly, the point is often put that British industry is hamstrung by regulations and crippled by taxation, and could do much better if these were removed.

Dealing with the last point first, there is no doubt that difficulties and frustrations are resulting from controls, and equally no doubt that most of them could not easily be removed in the present economic circumstances. But it is not possible to come to any general conclusion about either the effects of regulations or high taxation. When these were largely absent before the war, a new industrial revolution did not take place; on the contrary, industry tended to seek easy and safe profits in specialised markets and limit price competition which might have stimulated new developments. There is evidence to show that industrialists are more ready to invest when the Government is a right-wing one, but even this does not necessarily follow.

The higher cost of labour owing to the greater bargaining power of labour during times of full employment since the war may stimulate the use of more equipment in the long run, as it seems to have done in the United States. It may already have done so, though there is a great temptation for business to raise its prices to cover wage increases and not become more technically efficient, especially when industrialists believe, as some clearly do, that the Government will not be able to ensure full employment and that unemployment will soon make labour a relatively cheap commodity again.

The standardisation argument is the most interesting of all. Following up the argument of size, there is evidence to show that British firms are too small to produce as economically as they might do. This can be seen from the diagram. Supposing that the situation has not radically changed since 1935, by far the larger part of the labour employed in this country would seem to work in small firms with a relatively low net output per head, and 80% in firms with a net output per head below the national average. Greater size not only makes it possible to carry out operations on a larger scale, but also to spend more money on research. Unfortunately there are no national figures showing expenditure on research by size of firm, but a survey done by the Federation of British Industries in 1946 gives some idea of what the industrial research expenditure picture is like: 420 firms replied usefully to the F.B.I.'s inquiries—something under 1% of all the manufacturing firms in the country; these represented, it was estimated, about half of the firms actually doing internal research. About 1·6% of British firms were therefore doing research on their own, and spending about £30 million a year (1945–6), or two-thirds of 1% of the total annual value of British manufactures. Firms spending less than £1000 a year on research were not included, and most of the recorded expenditure must have been made by larger firms in which, the survey noted, "Scientific research is now an established practice . . ." All research does not, of course, mean productivity increases. It is quite possible, for example, to research into making a product *less* standardised. But supposing that most research is useful, and not merely designed to oust troublesome competitors, it might be an advantage to science if firms were bigger, provided that this did not make them into potential monopolies which might discourage innovations (as the Bell Telephone Company seems to have done in the United States). This, of course, ignores the possibility that the small firm may be enabled to take part in research it would

not otherwise be able to afford through such devices as co-operative research. How effective this is as a substitute for a firm keeping track of and developing scientific advances for its own use is not clear, and at present the sum of money involved annually is relatively small—just under £1½ millions in 1946 and just under £2 millions in 1947. The bulk of this was subscribed by Research Associations themselves, and the residue by the Government through the Department of Scientific and Industrial Research. In neither year was the full amount of income spent, which seems to indicate that small firms did not think that there was any very great urgency about the matter.* Co-operative research is certainly useful, but it seems unlikely to make small industrialists as scientifically-minded as if they were supervising the work themselves. To determine how much research a small firm can afford would be a useful piece of research in itself.



This diagram shows how British productivity varies with the size of the manufacturing unit; from the upper line productivity seems to rise as firms become larger, though when they become too large (employing over 7000 people) it tends to fall off again. The lower line, which shows the percentage of the total labour employed in firms of each size, indicates that most British firms are too small to achieve a peak output per head of over £300.

Against the argument for great standardisation of product and greater size of firm is the counter-argument that standardised articles are not what the consumer of British articles wants to buy, and that therefore the argument for greater mass-production is futile, since it would only leave us with unsold stock. The first pre-requisite of mass-production is, it is said, a large market for standardised products at home, and that we have not got. "In the United States," noted a former head of the F.B.I., "the reaction of a customer to a novelty is, 'I would like to be one of the first to try it'. Our reaction in Britain is, 'Something new? I would like to see how it works out somewhere else before I try it'." We are, without doubt, conservative in our tastes, but this, it can be argued, is more the fault of the industrialist than of the consumer. The market for a product is largely what the producer makes it. If this were not the case there would be no justification from his point of view for the large expenditure on advertising in this country. Every day markets are made for products where none existed before. But it is easier in many lines of production to sell a non-standardised product than a standardised one, and businessmen have been, it might be supposed, either too lazy to make the effort or too timid to take the risk.

On the whole, one is bound to conclude that though there are economic reasons why higher productivity in British industry should not have been forthcoming, those reasons might not have been operative if British business had been more willing to take risks, expand expenditure on research and plant, and encourage the public to accept less highly differentiated products. Far from doing this, it has made a virtue of keeping Britain's products as they are by emphasising their quality as compared with the shoddiness (*sic*) of standardised products. On occasion it has apparently made a virtue out of denying the results of research to the people of this country. Lord Stamp, replying to accusations that British industry was in the habit of putting patents "in the ice-box", argued at a meeting of the British Association, that it was not economic to introduce a new process before the old one was obsolete, and that unemployment might result. But how can scientific and economic progress, which demands a larger and larger supply of goods for the consumer, be achieved if this attitude is adopted? Economic change has to take place, even though it may involve some part of the public in risks. It seems that industrial experience has done nothing but make us economically cynical, whereas in the United States trade depressions and other industrial ills have failed to damp the optimism of either producer or consumer.

The fashionable thing to do when economic impasses of this kind arise is to turn to the Government. What can the Government do to improve the productivity situation and relieve the frustration of science?

All political parties have agreed that, if they are in office, they will keep up the level of investment (that is the amount of investment goods which are consumed at any one time), sufficiently high to ensure that everyone in the country has work to do. This can be done by the Government, in the last resort, buying investment goods itself and building new roads, bridges, dams, and so on when the situation demands it. But this is a different thing from ensuring permanently that investment is stimulated and guided into the channels that will increase the productivity of British industry. By and large, except in the case of the nationalised industries, this will entail the Government persuading private business to do this—a difficult matter. Devices such as the Capital Issues Committee and Develop-

Continued on p. 388

* Any firm may now by law be compelled to become a member of, and subscribe to, the funds of a Research Association, but this, of course, does not solve the problem, since it cannot compel any enterprise to take advantage of the facilities offered.

Aureomycin

AUREOMISIN

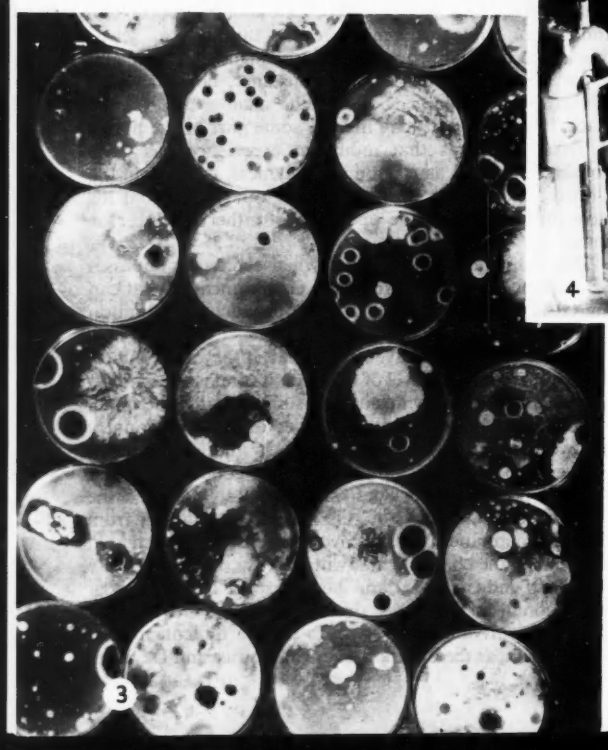
THESE photographs illustrate the creation of the new drug aureomycin—so named because it is produced by the mould *Streptomyces aureofaciens*—which has already proved sufficiently successful in clinical trials to justify its addition to the very short list of antibiotics which have established a place for themselves in general medical practice. The others on the list are penicillin, chloromycetin, and streptomycin; this total of four is a disappointingly small proportion of the many antibiotics which, in a literally world-wide effort, have been exhaustively investigated since the unique properties of penicillin were fully realised.

Like all successful chemotherapeutic agents, aureomycin owes its effectiveness to a combination of two properties: a high toxicity t a relative harr body.

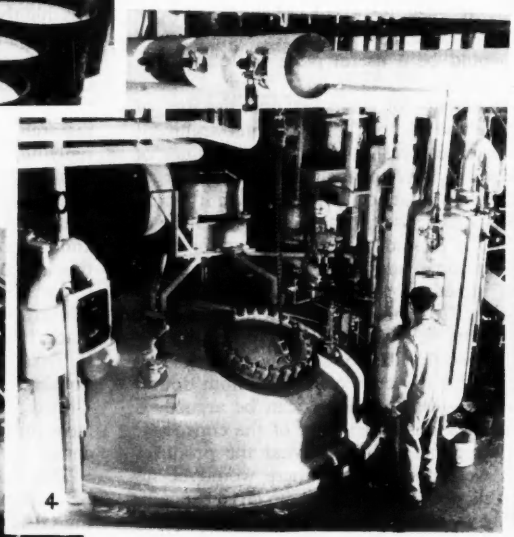
Aureomycin of penicillin-r among them a Fever and Q F quences—cause smaller than n of still smaller ordinary micro for example, caused by virus

1. Behind the discovery of almost every new antibiotic lies a story of weeks or months of unrewarded drudgery in the laboratory. The parent of aureomycin is a mould strain originally isolated from a sample of soil obtained as part of a survey which covered the whole of the United States. Altogether some 600 samples were tested; some of them are seen here undergoing preliminary examination by Dr. B. M. Duggar, discoverer of aureomycin, who is a research worker at the Lederle Laboratories in New York.

2. For fut tage of any e with the sam terised. Thi microscope t its habit of useful.



3. Jelly containing sufficient nutrients to support the growth of moulds was placed in flat glass Petri dishes and sterilised. Each dish was inoculated from a different soil sample and then set aside in a warm room. In a few hours mould colonies such as those seen here began to grow on the jelly. Pieces of each kind of mould colony were carefully picked off with sterile needles and transferred to tubes for further culture and experiment.



4. Not until the most careful laboratory experiments have been successfully completed, is it justifiable to proceed to clinical trial. Here the mould, derived from the original culture, is being grown in vats containing 9000 gallons of nutrient liquid—a vast magnification of the 5. Every stage of the refining process has to be carefully checked to ensure that the standard of purity essential for any drug which is eventually to be administered to human beings is being maintained. Here a sample for testing is being taken during the refining process. The final stage of isolation is when the precipitated and is separated from the liquid by the giant rotary suction filters shown here. The yellow crystals of aureomycin adhering to it are scraped off and fall into trays below, from which they are collected and distributed.

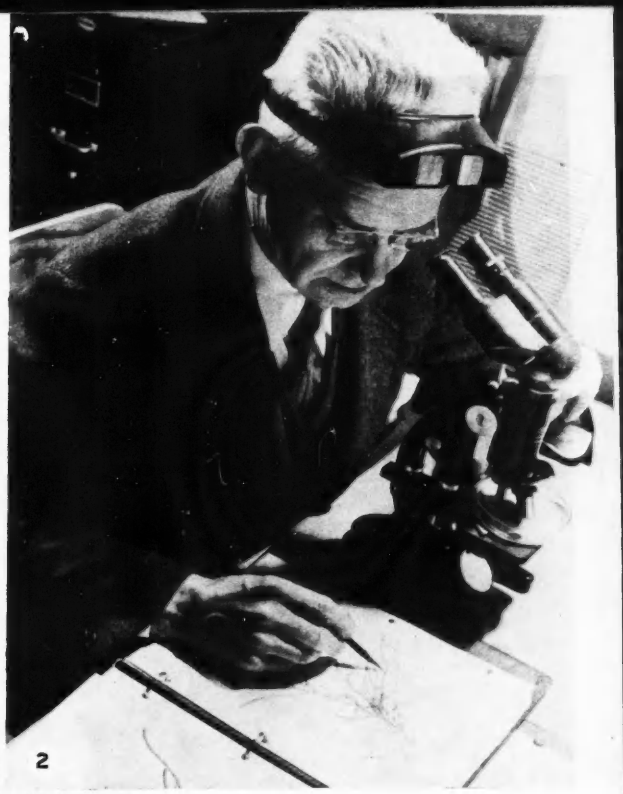
including cures of in production on a lar ratory cultures, is gnification of the checked to ensure to be administered final stage of iso ant rotary suction and fall into trays

A PROMISING NEW ANTIBIOTIC

high toxicity towards a range of infective organisms and a relative harmlessness towards the cells of the human body.

Aureomycin is valuable because it destroys a number of penicillin-resistant infections. Especially important among them are the diseases—such as Rocky Mountain Fever and Q Fever, both of which have dangerous consequences—caused by organisms called Rickettsias, which are smaller than normal bacteria. Certain viruses—organisms of still smaller size, so small indeed as to be invisible with ordinary microscopes—are also sensitive. Aureomycin has, for example, effected spectacular cures of pneumonias caused by viruses.

2. For future reference, and in order to take advantage of any earlier research which may have been done with the same strain, the mould is carefully characterised. This can be done by noting under the microscope the essential features of its structure and its habit of growth. Simple chemical tests are also useful.



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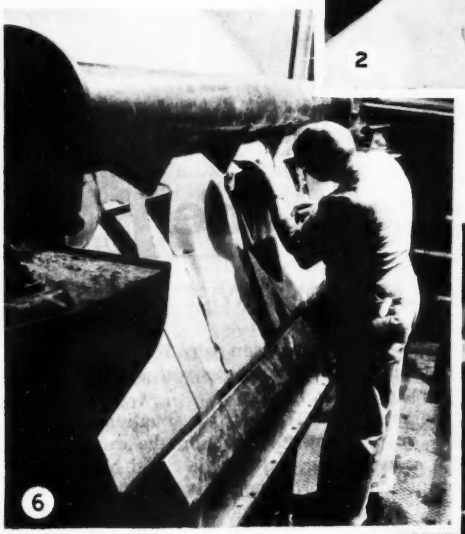

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tory experiments, including cures of infections deliberately induced in animals, justifiable to production on a large enough scale to carry out an extensive ed from the factory cultures, is seen growing in fermenting tanks, each liquid—a vast magnification of the test-tube experiments originally done. has to be care checked to ensure that the material reaches the high stan- which is event to be administered to human patients. Here a sample for ing process, the final stage of industrial production. The drug has been liquid by the ant rotary suction filters. As the filter rotates, the golden- to it are scraped and fall into trays, from which they are taken to be bottled

7. From the 600 soil samples were isolated over 3000 strains of moulds showing some promise of producing useful antibiotics. These strains were painstakingly isolated and grown as pure cultures on nutrient jelly in sterile test-tubes, a number of which are seen here. As the work progressed, more and more of the strains were eliminated, until finally the aureomycin-producer alone was subjected to really detailed examination.

7

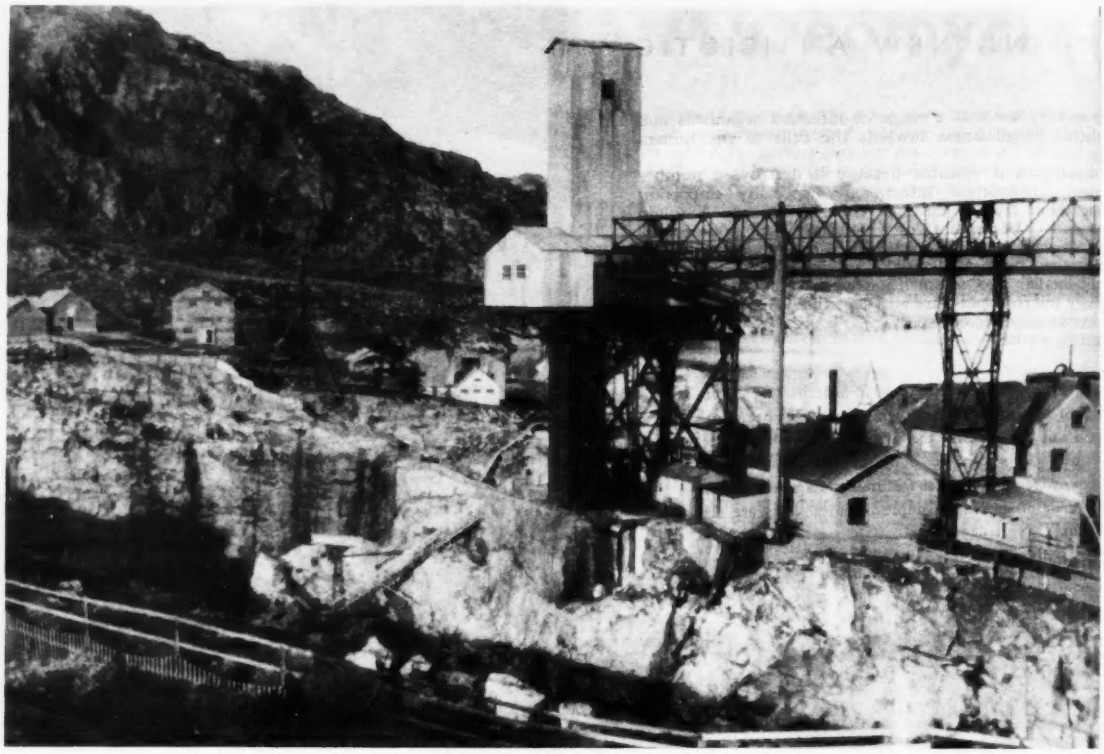

FIG. 1.—The mine which supplies the world with cryolite is situated at Ivigtut on the Arsuk fjord.

Greenland's Ice-stone Mountain

JOHN LANGDON-DAVIES

THE expansion of the aluminium industry has been so rapid that the present generation takes for granted a branch of metallurgy which, not much more than sixty years ago, consisted of little more than one unsolved and apparently insoluble problem.

Aluminium is a commoner element in the earth's crust than iron. Moreover, it is in many ways a more useful element, especially when we recall the obvious fact that gravity is man's hereditary enemy and a reduction of its effect to one-third would throughout human history have been desirable. Why then did not an Aluminium Age succeed to the Bronze Age instead of an Iron Age?

The answer is that whereas the reduction of iron from its oxide is a reasonably simple process, the reduction of aluminium defied the ingenuity of mankind—even the ingenuity of a Humphry Davy.

Various chemical techniques were attempted; but at such a cost that the small quantities of aluminium obtained remained in the category of semi-precious metals. Aluminium metallurgy of a type likely to have commercial possibilities had to wait for the perfecting of an electrolytic technique. Now the temperature needed to fuse aluminium oxide is in the neighbourhood of 2050° C. This was the reason why Davy was unable to repeat his electrolytic successes with sodium and potassium in the case of aluminium.

That was the situation in 1807. Davy established to his own satisfaction the existence of a 'metal of clay', but beyond this he could but add wistfully: "Had I been so fortunate as to obtain the metallic substance I was in search of, I should have proposed for it the name of Aluminium."

Thirty-eight years passed: the almost omniscient Friedrich Wöhler, hero of synthetic urea and, therefore, anti-Christ to the vitalists of his day, abandoning electrolytic dreams,* succeeded in isolating the metal by chemical means in a quantity and form in which its properties could be studied. That was in 1827, but it was not until 1845 that he succeeded in melting this aluminium powder to make a coherent metallic mass.

Wöhler reduced aluminium chloride with potassium, a process which was too expensive to please the Emperor

* Actually Oersted had succeeded in making metallic aluminium in 1825, though his technique did not lend itself to large-scale development. He heated aluminium chloride with potassium amalgam (potassium metal 'dissolved' in mercury) and then distilled off the mercury: the residue contained lumps of aluminium metal.

FIG. 2.—A view of the cryolite mine from the south.

Napoleon III, who had fallen in love with the new light metal at first sight, and had set his heart on aluminium helmets for his soldiery.

The Emperor was therefore moved to subsidise Henri Sainte-Claire Deville to find out a cheaper process. Deville used sodium instead of potassium and the price of the metal began to go down. But still aluminium remained in the semi-precious category. At the Paris Exposition of 1855, bars of the metal were placed symbolically near the Crown Jewels. The King of Siam was enchanted and on his departure from France was made happy with a watch-chain charm of the new metal. The price of aluminium fell, but only to £1 per pound.

Some eighty years after Humphry Davy was defeated by a temperature of 2050°C., Charles Martin Hall, a young American chemist, made the vital discovery.

Hall reasoned that if he could find a fused salt able to dissolve alumina—that is, aluminium oxide—in substantial quantities, electrolysis in solution would be possible. The intermediary that he found was cryolite, a double fluoride of aluminium and sodium, $3NaF.AlF_3$; this discovery he made in 1886. The melting-point of cryolite is 1000°C., about half that of alumina, and it dissolves alumina in substantial quantities. The modern commercial process is worked with a 16% solution of alumina in cryolite, which is molten at a temperature of about 935°C.

Prior to the momentous discovery which made possible the production of cheap aluminium by electrolysis, cryolite, or ice-stone, had been used in the soda and alum industry, but in 1871 new processes virtually put a stop to the employment in this way. The Danes, who watched with anxiety the collapse of their important chemical industry, could hardly have known that a new and far greater industry was on its way.

Charles Martin Hall is generally credited with the founding of the aluminium industry (although his method of making aluminium was discovered quite independently by the French chemist, Hzroult, in 1886) and the substance which he used to solve its major technical problem, cryolite, has remained to this day the only satisfactory one for the purpose. Without cryolite, it is quite possible that aluminium would still be a semi-precious metal.

The remarkable fact in this story is that cryolite can only be obtained in satisfactory quality in one quarry in the whole world. Though there are small deposits near Pikes Peak in the Colorado Rockies, and near Miask in the Urals, it is only at Ivigtut on the Arsuk fjord in Greenland that useful cryolite is mined. Thus, a seam 60 metres deep at present, and only able to be worked at certain seasons in the year, is an essential factor in the aluminium output of the entire world.

In a treeless, inclement desert of the far north, with a mean annual temperature close to freezing-point, a barren cliff reveals a perpendicular seam, mixed with iron pyrites, copper pyrites, galena, zincblende, and quartz. A fortunate accident of nature has sited the quarry near a good workable harbour, and, so long as the water does not freeze, operations can be carried out.

The cryolite, with many impurities, is transported to Copenhagen for refining, except for a percentage which under long-term agreements with the Danish Government goes direct to Pennsylvania for the U.S.A. markets.

The Kryolitselskabet Oresund A/S, ninety years old, deals with the entire output apart from the American share. The process consists of a preliminary rough sorting followed by crushing. Many of the impurities being magnetic, the cryolite sand is passed over magnets, and other impurities are separated out by a flotation process. Quartz is the last impurity to be removed, and for this, reliance is placed in the fact that cryolite will grind into fine powder more easily than quartz.

The aluminium industry absorbs 80% of the total output of cryolite, and requires a purity of 99·7%. Since the impurities may, to begin with, reach 25% of the unrefined mineral, it will be seen that a rigorous technique has had to be devised. Besides the aluminium industry, the chief customers for cryolite are the enamel industry (10 to 15%), where it has largely taken the place of tin oxide; the glass industry, (5 to 10%) where real opalescent glass (not ground glass, which is sand-blown) is made by allowing 1/1000 mm. crystals of sodium fluoride to separate out; the insecticide industry which incorporates cryolite in sprays for fruit trees, and in poisons for use against boll-worm, cut-worm, and bag-worm. The toxic principle in cryolite insecticide is derived from the fluorine content.

During the last fifty years over a million tons of raw cryolite have been mined in Greenland; about a third of that amount was exported to North America, and two-thirds went to Denmark. In the recent war almost 250,000 tons were exported to North America in the period (1940–44) when Denmark was cut off and did not receive anything; in those five years the aluminium production of the U.S.A. increased considerably and of Canada still more so. During the war refined cryolite was supplied from plants in these countries to Britain. The cryolite exported from Greenland during the period 1945–49 was disposed of according to the old proportion, and against an export to the U.S.A. of about 60,000 tons Denmark took about 100,000 tons.

99·5% of the cryolite that Denmark refines is exported, so that this one rare mineral plays an even more important part in Danish international trade than uranium and radium from the Congo play in the trade of Belgium.

Nothing but a lucky accident is likely to bring to light another cryolite seam—the present one was revealed by the action of the ice cap stripping off overlying layers of gneiss—so that the only competition which has to be feared is synthetic cryolite. Most large alumina reduction factories have facilities for manufacturing this, but in present circumstances the use of the natural substance is found economically more satisfactory. Borings which have been made suggest that the reserve supplies are sufficient to guarantee the market for many years to come.

The story of cryolite illustrates the vicissitudes, the element of luck, the unexpectedness, so often found in the techniques most essential to civilisation. Of the uses mentioned above, that in the aluminium industry is the most recent (save perhaps for its incorporation into insecticides), and yet it is that industry which not only uses it most but is most dependent upon it. By its effect on the aeroplane industry, aluminium transformed war, and it is a remarkable fact, therefore, that one seam of mineral in Greenland should have played such a crucial part in the evolution of human civilisation.

(The photographs in this article are reproduced by courtesy of Kryolitselskabet Oresund A/S of Copenhagen.)

SCIENCE, EFFICIENCY AND INDUSTRY—*Continued from p. 383*

ment Councils might help,* but if British business is disinclined to invest, it may not be persuaded to do so, however much guidance and assistance are given to it. Similarly, the Government may spend money on research itself (in the year 1947–8 it spent £69 million, all but £9 million on the Armed Services), and try to promote research and its application to industry through such bodies as the D.S.I.R., but this does not ensure that improvements will be adopted by manufacturing business. This difficulty has received expression in the recent setting up of the National Research Development Corporation, a Government-sponsored body with power to borrow up to £5 million which will "promote . . . the development or exploitation of inventions resulting from public research and of any other invention . . . not being developed or exploited or sufficiently developed or exploited", presumably by private industry. This suggests that Government-run manufacturing industry is being thought of as at least part of the answer to the problem. Perhaps this will act as a challenge to private firms. Meanwhile the future of applied science and productivity in this country still rests primarily on the decisions of thousands of business-men—on whether or not they will spend more on research and on the re-modelling of their firms on up-to-date lines. Without doubt industry is becoming more scientifically-minded. In 1946 it was spending twelve times more on research than in 1930, and about four times what it spent in 1938. Productivity, we are assured, is rising as a result of the efforts of the past two years. But the process is a slow one, and the immediate need for progress is very great.

* The approval of the Capital Issues Committee is required before a firm can raise money by asking the public to buy its shares. Development Councils were referred to in the previous article.

READING LIST

The following references will help readers who want to study the comparison of productivity further:

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Natural History of the Skuas

RICHARD PERRY

No country is more fortunate in its wealth of sea-bird colonies than the British Isles. Not the least interesting of these sea-birds are two species of skuas, which reach their most southerly breeding limits in the extreme north of Scotland. It must always be a matter of personal opinion, I suppose, as to which subject offers the most fascinating and satisfying field of study—the micro-organism, static under the high-powered microscope, but observable whenever desired, or the macro-organism, in this instance a colony of sea-birds, whose command of flight prevents the field naturalist from ever observing their behaviour, even during the breeding season, as fully as he could wish. The latter can, as I have done on three occasions, put in six months of more or less continuous observation at a sea-bird colony: as continuous as his mental and physical resistance to the rigours of the British climate permit; but once the breeding season is at an end his subjects spread their wings in mockery of his patient watching and are lost to him for another six or eight months. Not quite lost, however, for some of them bear numbered aluminium bands upon their legs, and these he may hear more of at some future date, while others, though not perhaps the selfsame, he may observe in their coastal and oceanic dispersal during the autumn and winter and the prolonged wanderings of immaturity.

What then can be learnt of the life-history of an individual sea-bird and the behaviour of a group of sea-birds, as instanced by the Arctic skua (*Stercorarius parasiticus*), known in America as the parasitic jeager, and the great skua or bonxie (*Stercorarius s. skua*)? A comparison between their respective life-histories provides fascinating material for speculation on a variety of problems.

Such speculation begins right away when we note the very wide divergences in their geographical distribution. Thus the Arctic skua is found breeding extensively throughout the northern hemisphere above the 55th parallel; whereas the bonxie is restricted to Britain, Færoe, Iceland, and possibly Greenland.* In Iceland there are a few thousand pairs of bonxies, but nowhere else do they number more than a few hundred pairs—there are less than one thousand pairs in Britain. It must, therefore, be relatively an extremely scarce species, though allied races, almost identical in appearance and behaviour, inhabit the southern hemisphere.

In Britain the Arctic skua breeds in the extreme north of the Scottish mainland, the Hebrides, Orkney and Shetland: the bonxie in Orkney and especially Shetland only.

What are the factors, then, that restrict the bonxie's distribution over the remainder of the northern hemisphere? Both skuas favour predominantly rough moorland and boggy ground as breeding places, at which both nest in loosely knit colonies, though in Finland the Arctic skua nests singly on small islets. Both lay the same number of eggs. Neither is victimised by any other species of bird. Both employ similarly aggressive tactics in mobbing intruders on their nesting grounds. Both are mainly dependent on the same source for their food supply—the presence of neighbouring sea-bird colonies.

It would seem, therefore, that we must look to the past for a solution of this problem, for it is significant that there are no races of Arctic skuas in the southern hemisphere, despite the fact that they winter mainly in this hemisphere. We may, therefore, speculate as to the possibility of the bonxie being a comparatively recent introduction into the northern hemisphere from the original southern stock, and still in process of 'finding its feet' as a northern breeding species.

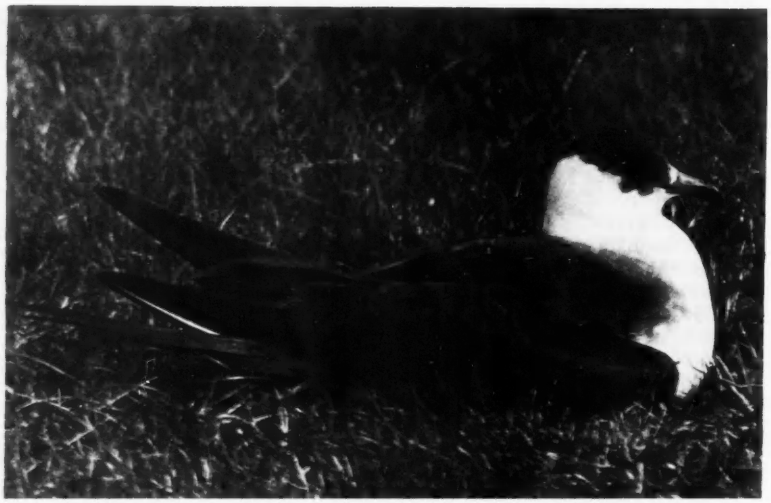
During the autumn and winter, then, these two skuas pursue a vagrant life, commonly solitary or in twos and threes, over the vast expanses of the north and south Atlantic respectively. The course of their wanderings is ordered mainly, no doubt, by the movements of those other species of sea-birds, whom they both rob of fish and prey upon. In the spring, however, the adults respond to the urge—possibly induced by a seasonal regrowth of the gonads—to return to their breeding grounds. By what sense they are apprised of the precise position of these and by what means they are able to orientate to them, we are at present totally ignorant. The majority of the immature skuas, however, probably continue their oceanic wanderings for some years.

Meanwhile, the earliest breeding bonxies and Arctic skuas had arrived on Noss in Shetland (where I carried out my study) in the first and last weeks of April respectively, and by the end of May I had completed the tally of 113 breeding pairs of bonxies and 31 pairs of Arctic skuas. There has been a colony of the latter on Noss from the earliest times, and it may, doubtfully, have included as many as 200 pairs thirty or forty years ago. The first pair of bonxies, on the other hand, did not reach Noss until 1910. Today, the latter certainly seem to be squeezing out the little skuas, but we cannot definitely state that their decline is solely due to pressure by the bonxies, for Arctic skuas, like terns, are subject to periodical alternations in their breeding grounds.

In addition to the breeding pairs of skuas, six or seven pairs of non-breeding Arctic skuas, some of which did not arrive until late in June, took up territories and held them for many weeks; while more than fifty apparently aged bonxies, which began arriving in the middle of May, formed a social gathering on the flat top of one of the island's low hills, and sat there daily until late in July, when they responded once more to that impelling urge to wander.

This partiality for a prominent eminence proved to be a feature of the bonxie's breeding behaviour, for on their return each pair took possession of a grassy or mossy mound raised a little above the surrounding waste of moor or bog. These mounds were obviously traditional, and were sited from 15 yards to 130 yards apart (58 yards on an average). Around them all breeding activities centred. In some instances, where an eminence had only recently been

* In Europe the Arctic skua breeds from Jan Mayen, Spitzbergen, Franz Joseph Land and Novaya Zemlya, south to Iceland, Færo, British Isles, Norway, Sweden, Finland, the Murman coast of Russia and the Divina Delta.



An Arctic Skua incubating.
(Photograph by G. T. Kay).

occupied and was not yet trodden out, the nest itself was actually constructed on the mound, but normally it was placed up to 44 yards from it among the long bog-grass or *sphagnum* moss. To its mound the bonxie moved when it was flushed from its nest—and every bonxie invariably rose from its nest at the distant approach of a human figure on the breeding grounds—and upon it off-duty mates stood or sat. On it, it alighted on returning from hunting or bathing. On it, it courted and mated, and from it launched its devastating attacks on any intruder. On it, or close to it, its young were fed.

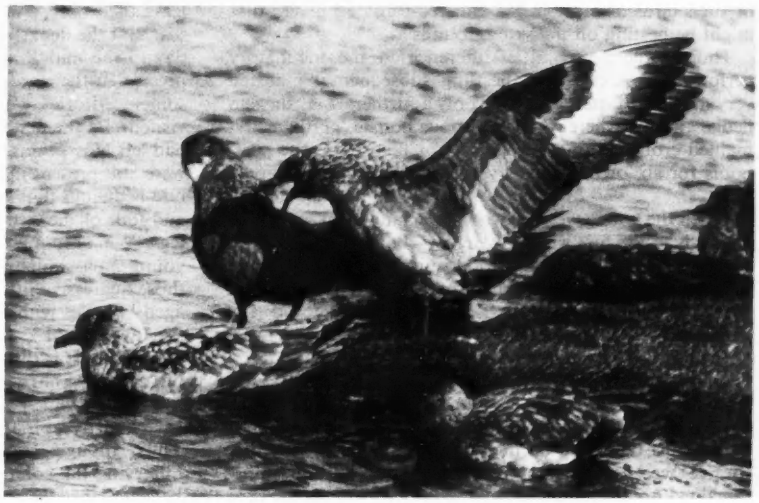
The Arctic skua does not demand such prominent look-outs as the bonxie, but it too shows a tendency to make a standing place on any little eminence that may be available. On Noss there was an average distance of 77 yards, and not less than 30 yards, between these standing places, with the nest—a few wisps of bent—up to 25 yards distant. As in the case of the bonxie all operations were conducted from this eminence, including its mobbing attacks, which were no less severe despite its much smaller size.

This mobbing of all intruders—beast, bird or human—is not, of course, restricted to the skua family. Terns make quite severe attacks with their beaks. Indeed, after a series of pecks from an Arctic tern—the most vicious of its kind—I have had blood streaming down my face from a scalp wound. Individual gulls will also swoop menacingly, though seldom actually striking one's head. Among the skuas, however, this mobbing is much more widespread and persistent, though here again it varies in severity from one individual to another. Some, indeed, never mob at all, merely circling impotently overhead. The majority, however, swoop threateningly down on the intruder, some striking him so severely about the head and face with their webbed feet that prolonged observation in their vicinity becomes an intolerable torment. But, while both species of skua have this habit it does not seem to be the same stimulus that prompts them to mob. Thus 80% of the bonxies began mobbing me on the day they laid their first egg, or within three days before this climacteric, their attacks increasing in severity as the eggs became hard-set and reaching their height during the first ten days after the hatch. But little more than 20% of the Arctic skuas mobbed me during incubation, and then not commonly until half-way through the incubatory period, while nearly half of them never made any attacks; their main period of mobbing began when the young hatched.

There is a good reason for this difference in behaviour. The phenomenon of 'injury-feigning' among many species of birds will be familiar. In most species this is induced by the hatching out of the young, but in the majority of Arctic skuas this reaction was set up on the day the first egg was laid. This climacteric induced a nervous condition, in which, on any intruder approaching the nesting territory, the parent would run crouched along the ground, with wings rocking from one side to the other, or more commonly stagger about drunkenly in an upright posture, hissing and screaming the while. In extreme cases the bird appeared to pass into an almost cataleptic state, allowing the wind to blow it off its balance, it legs being stiff and unflexed. This was 'injury-feigning' in its most complex form, but it did not seem to me to include any conscious luring-away design. I would place it in the same category of nervous phenomena as that induced in the rabbit by the hunting stoat. It is odd that the great skua never exhibits a trace of this condition.

Once again, there was wide individual variation both in the intensity and also the pattern of the 'feigning'. Three pairs, for example, never 'feigned' at all, to my knowledge—though they began mobbing me as soon as their chicks hatched—and these three were, perhaps significantly, the only pairs whose eggs I failed to locate. Five pairs did not 'feign' until their young hatched, and one not until the latter fledged. Some only reacted to my presence on three or four occasions throughout the summer.

So much for a brief account of the skuas' behaviour on their breeding-grounds. The actual results of their reproductive efforts were very similar. The 31 pairs of Arctic skuas laid 57–61 eggs, the 113 pairs of bonxies 219–223 eggs. The latter hatched 70% of their eggs, the Arctic skuas 78% of theirs, with the bonxies finally rearing 114 fledglings, or approximately 1 per pair, and the Arctic skuas 29, or slightly less than 1 per pair, with a further



Great Skuas at bathing pool. Note the display posture with wings raised.
(Photograph by C. A. Gibson-Hill.)

small mortality among fledglings before they actually left the island.

The reproducing of their kind is, after all, the biological purpose which has brought these skuas to Noss from ocean parallels hundreds or thousands of miles distant. In what manner is their purpose achieved? The first three or four weeks after their homing are occupied with the setting up of territories—the majority, at any rate, return in pairs—and with the sexually inspired antics leading up to the mating and, finally, ovulation. Then, for 28–32 days in the case of the bonxie, and 25–26 in that of the Arctic skua, the embryo develops within its shell, while the parents incubate continuously. At the end of this period the embryo begins to exert pressure, fracture lines appear on the shell, and the chick may be heard cheeping within; but, before it succeeds in rupturing the envelope and forcing up a few minute flakes of shell, the bonxie chick must work away for a period varying from 19–73 hours, and the Arctic skua chick for 17–72 hours. Once a sizeable hole has been chipped through the shell, the chick rests for some hours, so that before the whole shell finally collapses under pressure, a further period elapses of from 10–67 hours in the case of the bonxie and 7–68 hours in that of the Arctic skua. Thus, this struggle to emerge may involve a bonxie nestling in from 36–85 hours intermittent exertion, and an Arctic skua nestling in from 19–120 hours, while in one extreme instance one of the latter emerged as a perfectly normal nestling 171 hours after first fracturing the shell!

Once hatched and dried off—a process which took between 12 and 24 hours to complete—the behaviour of the respective nestlings was very similar, for the dominant impulse of both was to get out of the nest at the earliest possible hour. This they would succeed in doing within from 24–36 hours. The two chicks of a clutch did not hatch on the same day, so the two were never to be found together in the nest 36 hours after the first had hatched, nor were they found close to one another after both had left the nest. Moreover, if they were placed together they would peck at one another fiercely. Not until they were three weeks old were some of them to be found associating together once more. Whatever may be the reasons for this antipathy to nest and fellow, such behaviour actually led to a considerable mortality—as much as 80% of the total casualties—among those nestlings that wandered into the territories of neighbouring pairs, where they would be savagely pecked; for by the time they were three weeks old some had wandered as much as 200 yards from their nests. However, their parents had no difficulty in locating them at feeding time. One or other bonxie parent was indeed always keeping guard on the mound until the young were at least seven weeks old, though both Arctic skua parents might desert theirs for considerable periods when they were only ten days old.

Were it not that the Arctic skua is a widely distributed species, one would be tempted to suggest that the bonxie's comparative scarcity is due to the complex routine which must be followed before the nestlings can be fed; for the latter's nestlings at any rate, and possibly the Arctic skua's also, are fed exclusively on whole fish. But the fish must first be caught by a totally different species of sea-bird, which has secured it for its own young. That bird must then be sighted by a hunting skua, chased, and forced to disgorge. The skua must retrieve the fish before it drops to the water, or some other skua or gull will filch it. It must then swallow it itself, before finally disgorging it to its own young, after a return journey of perhaps several miles.

This not only *sounds* a highly complex and specialised operation, but to all appearances actually *is*. It is a matter of fact that, watching from the cliffs of Noss, I never witnessed an Arctic skua bring off a successful act of piracy against one of the auks—its main victims where terns are absent—throughout the summer; while only about once in ten times did a bonxie succeed in forcing a gannet to disgorge and, even then, quite often failed to retrieve the fish itself. However, as there was no evidence that skua nestlings ever starved, I could only conclude that both skuas were more successful in their piracy at the main fishing grounds, 20 or 30 miles offshore. For their own food neither of the adult skuas were wholly dependent on piracy. The bonxie, for example, killed numbers of kittiwakes and the young of other sea-birds, while both relished the eggs

of such birds as eider-ducks and red-throated divers that might be nesting on the skua grounds.

Though there was little difference in the incubatory periods, the Arctic skua nestling subsequently matured very much the faster, requiring only a further 27–33 days to fledge and fly, whereas the enormous bonxie 'nestlings' remained crouched in the bog-grass or floating in the peat-pools for six or even eight weeks, before they attempted flight.

When the young were between seven and eight weeks old —that is in the first week of August—both skuas began to be less regular in bringing food, and whenever an adult flew in, the nestlings of either species might be observed 'dancing' frenziedly before it, waving outstretched and decurved wings. This hunger dance was especially interesting because its antics resembled very closely that 'injury-feigning' antic which, as we have seen, is peculiar to the Arctic skua alone.

Before they set out on their oceanic wanderings the young skuas must be strong on the wing. So, for a further three to five weeks after first essaying flight, both nestlings remained on the island, though an occasional Arctic skua would make a short flight out over the cliffs and back. The young gannets, auks, fulmars, kittiwakes—all these I saw leave their nests and make their first flights down or out to sea. But the departure of the young skuas eluded me. One day in the middle of August they and their parents were present in their accustomed places; but the next morning some territories would be empty and would not be re-visited. By the first week of September all the Arctic skuas had left Noss, though two young bonxies and their parents were still present in the first week of October. Of the 135 odd fledglings that left the island all but two of the bonxies and four of the Arctic skuas bore numbered rings on their legs. What are my chances of hearing of them again, these young skuas which I have handled and watched grow from those aggressive little nestlings into these splendidly plumaged beauties with their magnificent powers of flight? Up to now only 1% of some 750 ringed skuas have been recovered. It is three years since I ringed my young skuas, but thus far I have received news only of one seven-month-old bonxie, which alighted exhausted on a French fishing-smack off West Flanders in the January following its birth. Thus, though I cannot experience the satisfaction of observing the complete life history of my small organism beneath the microscope, I am compensated to some extent by the gambler's stimulating suspense as to the future.

Joseph Black (1728-1799)

JOSEPH BLACK published only four works. Two were of minor importance: one (posthumous) was one of the best chemical textbooks ever produced; the fourth marked the beginning of the chemical revolution. A further piece of research, which he did not even bother to publish except through lectures to his students, produced a second revolution—this time in the science of heat. There can be few scientists who have fired so few rounds and scored so high a proportion of bulls.

Black studied medicine at Glasgow and Edinburgh. His doctoral thesis, *Experiments upon Magnesia Alba, Quicklime and some Alcaline Substances*, presented in 1754 and published in expanded form in 1756, has a better claim than any other work to be called the beginning of modern chemistry. The central point of the researches which he reported was the discovery of carbon dioxide, or to be accurate the first clear realisation and proof that it was a gas in its own right and not a mere modification of atmospheric air, as all gases had hitherto been thought to be. As Sir William Ramsay said, "Before Black the word *gas* had no plural."

Black's discovery of carbon dioxide was the beginning of pneumatic chemistry, the line of research that led through Priestley and Cavendish to the consummation of the chemical revolution in the work of Lavoisier. Black obtained the gas, which he named 'fixed air', by the action of heat on magnesium carbonate and on chalk (calcium carbonate). He was then able to explain the relations and differences between chalk, quick-lime and slaked lime in terms of the addition or subtraction of 'fixed air' and water; and the parallel differences in the case of magnesia and the alkalis. His results depended on the use of the balance, and this work, probably more than any other, served to establish the importance of quantitative methods in chemistry.

Appointed professor of chemistry and of medicine at Glasgow in 1756, Black resumed an earlier interest in heat and undertook the researches which led him to the two discoveries on which the science of heat is based: first, that each substance requires a characteristic amount of heat, its 'specific heat', to alter the temperature of a given quantity by a given amount; and secondly, that to produce the liquefaction or evaporation of any substance it must be supplied with a definite amount of heat, 'latent heat', which has no effect on its temperature. This work was never published during his life, but became widely diffused through his lectures. It marks the point of transition from a mere science of thermometry to a science of heat, and so is the beginning of the second revolution to which we have referred. It also had immediate practical consequences, for it was by means of the concept of latent heat that James Watt arrived at the idea of a separate condenser as the basis of an efficient steam engine.

Black also showed many other interests—his correspondence reveals, for example, that he constantly gave advice on industrial chemical problems. In 1766 he moved to the chair of chemistry at Edinburgh, where he continued to teach till his death on December 6, 150 years ago. He was specially renowned as a teacher; his pupils made many important contributions to chemistry, and it was chiefly through them that Black's work came to influence the other giants that stand between him and Lavoisier in the preparation for the chemical revolution.

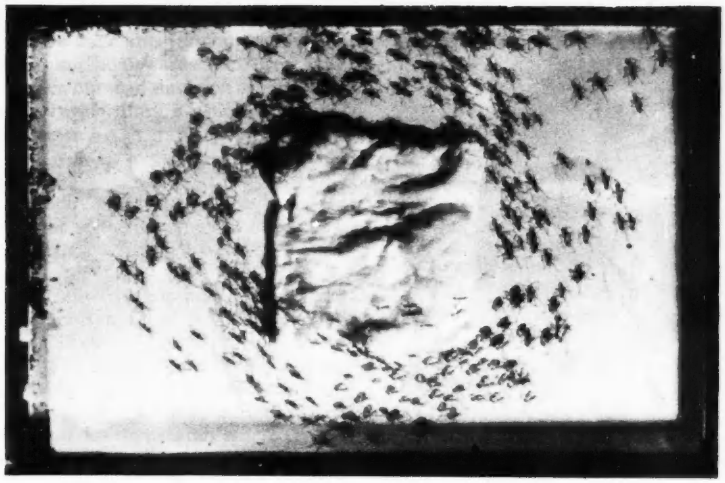


FIG. 1.—These Army Ants (*Eciton hamatum*) have been induced to circle in a ring inside a laboratory nest. The circling can continue for a week or more.

(*Photo by T. C. Schneirla*).

Army Ants on the March

DEREK WRAGGE MORLEY, M.A., F.L.S.

DURING the recent war many soldiers fighting in Africa and on the borders of India saw for the first time the dreaded 'Isanafu' or Driver Ants. These African and Indian army ants which belong to the ant genus *Dorylus* have provided the meat for travellers' stories ever since Europe first had contact with India and 'Darkest Africa'. The ants march in long columns which may be a quarter of a mile long. The sides and front of these columns are guarded by large-headed worker ants known as 'soldiers', which have jaws half a centimetre long.

The ants sometimes march at night and sometimes during the daytime, according to the species involved. But whether it be night or day they will march straight ahead, through houses or native huts, crossing streams by means of bridges formed of their own bodies, and eating all small insects and animals that remain in their path. Even a horse will be eaten and left a skeleton if it is tethered in their path and unable to escape. The invaders are not unwelcome, however, and the native population likes them because they clear huts and houses of mice and other vermin—even of other species of ants. These army ants of India and Africa are relatively well known owing to the many popular accounts which have been written of their activities, but few people in Europe know that a similar group of ants which belong to a different but closely allied genus (*Eciton*) are common in the southern part of the U.S.A. and in central and South America. Both *Eciton* and *Dorylus* belong to the same family (Dorylinae).

The habits of these ants were first described by Bates in his book *The Naturalist on the Amazon*, published in 1863. Like their African counterparts, whose activities were first described by Savage in 1847, these ants spend much of their life on a series of predatory marches. During the period of marching they form only temporary nests or swarm-clusters when they halt. They may stay in the same spot for two or three weeks during the stationary phase but they do not usually stay for more than a few days in any one place when they are on the march.

This alternation of 'statory' or stationary phase and a marching or 'nomadic' phase has long puzzled biologists. Belt thought that the ants exhausted the supply of food in each locality within two or three weeks owing to their intensive hunting activities, and, therefore, had to move to new hunting grounds, foraging within the immediate vicinity of their march as they moved. Thus he says: "It is a curious analogy that, like the hunting races of mankind, they have to change their hunting grounds when one is exhausted and move to another." This theory was accepted, although scientists were somewhat sceptical, until the work of T. C. Schneirla and his colleagues.

It was in 1932 that Schneirla commenced his studies of *Eciton hamatum* and *Eciton burchelli*, two common species of Central American Army Ants, on Barro Colorado Island, Panama. Schneirla found that these stationary and marching phases are closely related to the reproductive functions of the *Eciton* queen, which are of a peculiar monstrous type only found in the Army Ant family (Dorylinae).

In studies of *Eciton hamatum* he found that at regular intervals (of approximately thirty-five days) the abdomen of the queen swells up, and within a few days of the start of this swelling she releases an enormous mass of over 20,000 eggs (Fig. 3). After this the queen remains contracted and in a resting state. The time taken by this cycle varies according

FIG. 2.—A day in the life of a swarm of Army Ants (*Eciton burchelli*).
1. In the early morning the Ants start out from their bivouac.
2. There are now two largish swarms some way from the nest with two main swarms connecting the swarms and the nest.
3. In the late morning there are more ants returning than going out. There is much less activity amongst the ants at this time of day.
4. Later there is resurgence; two main trails develop.
5. At nightfall a new bivouac is formed; ants are drawn into it from the old bivouac and all the trails. Activity along the trail comes to a virtual standstill during the night.

to the humidity and temperature; in the rain forest zone of Panama, it only lasts twenty days.

The nomadic marching phase commences as soon as these eggs reach the larval stage and it ceases "precisely when this brood has become mature and is mainly enclosed in cocoons". In the stationary period colonies are found to contain broods in the pupal 'cocoon' stage, and this period ends again when this brood of pupae emerge as callow workers. While this cocoon stage is occurring, the queen lays her next lot of eggs. This happens about seven days after the start of the stationary phase, and their development into active larvae coincides with the hatching-out of the cocoon-brood into callow 'new-born' workers and soldiers. Schneirla explains this correlation on the basis of the stimulation of the workers caused by the presence of active larvae demanding care and food and giving in return their much prized secretions to the ants which lick and feed them.

Schneirla and his colleagues carefully studied the way in which these two species of *Eciton* set out on their raids and found some interesting differences. In *E. hamatum* the large and small workers form a series of trails when raiding, usually two or three in number; in *E. burchelli* on the other hand, a fan-shaped swarm of ants is formed at the head of the raiding party, although the workers following behind this mass may use several well-defined routes.

Progress of a Raid

How does the raid start? In *E. hamatum* raiding activity begins with the first daylight, and by late morning two or three trail systems are usually well populated with ants. At midday there is a marked reduction of activity, but this ends when in the early afternoon there is a renewed resurgence of ants from the bivouac (as the temporary nest is usually called). These new recruits tend to favour one of the trails in preference to the others, and Schneirla thinks that this is determined by the degree with which the routes are saturated with the nest odour of the ants, or else with the odour of booty, the most odiferous trail being the favourite one. The accessibility of a trail and lack of physical barriers, or even the scarcity of spoil-laden returning ants, may also be factors which affect the choice of the trail along which the new resurgence takes place.

The rush of ants up the most favoured trail is continuous, and ants returning up the other trails are often drawn into the new outward surge directly they reach the nest, or sometimes even before they enter it. The ants which are still in the temporary nest become progressively more excited by the outward rush of their nest-mates up the trail, and are themselves gradually drawn into this outward surge. The growth of this mass exodus soon starts to block the return of the incoming ants, and gradually forces a more or less complete orientation of the traffic outwards up the favoured trail. This movement continues until all the ants have been evacuated from all the other trail systems and also from the bivouac into the favoured trail. The whole colony then moves down this trail until nightfall when a new bivouac is formed some 200 to 300 metres from the old bivouac.

In *E. burchelli* events are different (Fig. 2). The exodus from the bivouac begins at daybreak as in *E. hamatum*, and soon the ever increasing number of the ants takes on a fan-shaped form, moving steadily away from the temporary nest site. The direction of movement is maintained with little alteration once it is established and as the swarm moves away from the bivouac, a gradually tapering mass of tracks is seen to form behind it joining it to the bivouac. In the late morning there is an increase in the number of ants returning with booty, and it is not until after the midday lull, during which there is little activity, that the fan is reformed by an ever-increasing rush of ants from the

bivouac. As in *E. hamatum* these ants leaving the bivouac collide with those returning homewards and eventually force them to join their own outward direction after a short period of tension and struggle, during which the whole scene resembles an immense living traffic jam. The new bivouac is formed shortly after nightfall at about 100 metres from the old nest.

When a nomadic colony remains in the same place for more than one day it is due to the interruption of the flow of the afternoon rush from the nest through weather or other physical causes.

The saturation of the trails with the nest odour of the ants seems to be a very important factor in influencing the movement of these ants. Thus any member of the colony can readily follow the trails of each part of the raiding system without ever having been over the route before. When workers were removed from the bivouac and placed beside any trail made on that day they readily entered it and ran along the route even when no ants were travelling along it at the time.

If *Eciton* workers were held captive overnight while the colony moved to a new locality, they were able to follow easily any fresh trail made during their captivity by the rest of their colony when they chanced on it.

The ants will also follow trails of different, but closely allied species although they will do so more hesitantly than when they are on the trails of their own species.

Schneirla points out that this is in accordance with the experimental evidence for the existence of distinctive nest odours in ants demonstrated by Fielde (1902) and the present author (1942), and that as pointed out by the latter, there is evidently a marked similarity between nest and trail odours of closely related species.

Most readers will remember Fabre's processional caterpillars which followed each other round and round in a circle for many hours. Similar behaviour has been recorded in army ants of the genus *Eciton*, both under natural conditions and in the laboratory. Wheeler kept a colony of army ants in his laboratory, placing them inside a cylindrical jar on a stand surrounded by a moat. They at first marched up and down the sides of the jar, but then when he removed the lid, they started to march round the bottom of it. They formed a completely closed circle round the bottom and marched head to tail in procession for forty-six hours!

Schneirla describes an even more remarkable procession which he found outside his laboratory on Barro Colorado Island. At about 7.30 a.m. he found a singular group of *Eciton praedator* milling around near the cement wall outside the laboratory. The ants were circling in a ring, with a rather irregular outer border of straying ants in ragged file. The mill seemed at times to be composed of *two* main groups of ants circling more or less independently one inside the other, although at times all the ants appeared to move together. By 8.30 p.m. the group of ants had somehow separated into two quite distinct rings, probably due to the effect of a heavy rainstorm at 4 p.m. These rings were both circling anti-clockwise, and their outer edges were about 20 centimetres apart. Both were appreciably nearer the wall than the single ring before the rainstorm. At 10 o'clock that night both rings were still circling, but very slowly. By 6.30 next morning little or no circling was to be seen, but the entire area was littered

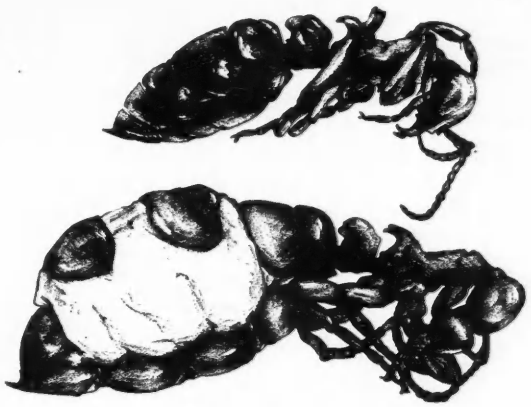


FIG. 3.—The lower drawing shows a queen of *Eciton hamatum* in the gravid condition. The other drawing shows a queen in the contracted state after egg laying. (*Drawing by Janet Roembild, from the 'Journal of the New York Entomological Society', Vol. 52*).

with dead or dying *Ecitons*, and an hour later virtually none of the ants were on their feet and various ant scavengers were carrying away the dead. The *Ecitons* had in fact died of desiccation after circling over a very dry area for more than twenty-four hours. To quote Schneirla's own account, "They might have been able to survive much longer had they not shifted their ring away from the wet each time rain broke during the day."

Since then Schneirla has frequently induced artificial circling by *Ecitons* in the laboratory (Fig. 1). This can easily be done since *Eciton* workers always tend to move along a surface which offers weak tactual stimulation. Thus when *Eciton* workers running over a table-top have settled, as they will, into a course along the outer edge of the table, a smaller circular column of them may be set up by placing a glass jar of 10 to 25 centimetres diameter near the table edge so that some of them brush it with their feelers or legs as they pass. "Ants that touch the jar lightly in this manner tend to leave the table edge trail and follow the jar round its circumference, and a circular column results which in the end may absorb nearly all the strays on the table."

Recently two colonies of *Eciton hamatum* and *E. burchelli* were brought back from Panama to the American Museum of Natural History in New York and placed on show in an apparatus of plastic tubes. They aroused great interest although they did little more than march backwards and forwards through the tube, with occasional stops to feed on mealworms until their death after five and nine weeks respectively. It is perhaps a pity that this exhibition could not have been made the opportunity for some simple form of teaching about the results of these most interesting studies, which demonstrate once more, and most effectively, the great difference between the findings of careful scientific study and experimentation and those of naturalists making unverifiable observations on animal behaviour.

The amateur with the right approach can give invaluable help to the expert, but the amateur who cares not a jot for science and scientists, but 'knows his animals', rarely produces any observations that can contribute to an accurate interpretation of their behaviour.

Far and Near

Juvenile Lectures on Photography

A SERIES of three Christmas lectures, specially adapted for a juvenile audience of from twelve to seventeen years of age, will be held in The Royal Photographic Society's House, at 16 Princes Gate, London, S.W.7. (close to the Albert Hall), on Wednesday, Thursday and Friday, December 28, 29 and 30, 1949. Each lecture will commence at 3 p.m. and occupy approximately one hour.

The lecturers and subjects will be as follows:

December 28, "Photographing People", by Mr. Richard N. Haile, F.I.B.P., F.R.P.S.

December 29, "How a Photograph gets into Print," by Mr. F. H. Smith, F.R.P.S.

December 30, "Photographing Marine Life", by Dr. Douglas P. Wilson, F.R.P.S.

Admission will be free but by ticket only, obtainable on application to the Secretary of the Society at 16 Princes Gate, London, S.W.7. It is requested, however, that such applications be made only by, or on behalf of, juveniles.

Bernal and the British Association

THE Council of the British Association decided last month not to re-elect Prof. J. D. Bernal as a Council member. His re-election had been deferred by the B.A. general committee when it met at Newcastle because press reports of his speech to the 'Peace Conference' in Moscow made it appear that that speech had included a strong attack on British science. (The fullest version of the speech published in Britain appeared in the *Manchester Guardian* of September 2. According to this Prof. Bernal had said in Moscow that "in capitalist countries the direction of science is in the hands of those whose only aim is to destroy and torture people. . . . They show this by their choice of weapons . . . of mass destruction meant for destroying the houses and fields, for the poisoning of men, women and children. . . . In the United States it will soon be the case that no one who is not and has not always been an open enemy of the Soviet Union will be allowed to teach or do research in science, and Britain will obediently follow suit. . . . Under capitalism science can bring no happiness, but only destruction. The scientist has no freedom; he is a slave to masters who have lost their reason.")

The British Association gave Prof. Bernal the opportunity to provide a copy of his full speech. The British Association states that "Prof. Bernal replied by sending the full text of his speech, together with a letter in which he recognised that one passage in the reports which had reached this country left room for misunderstanding, and assured the council that it had never been his intention either to state or to imply that the scientists responsible for the detailed direction of research in this or any other country were haters of peace. These documents were brought before the council at its meeting on November 4. The council recognises the necessity of distinguishing between political statements of its members with which it has no concern and statements on the direction and use of science in this country, and the objection which it took last August was solely to certain statements of the latter kind. These appear in Prof. Bernal's version of his speech, no part of which he withdraws, and in the view of the council its objections are not removed by his letter.

"Accordingly, the council has decided not to re-elect Prof. Bernal to its membership."

Prof. Bernal has since commented on the Association's decision, and accused it of supporting a policy aimed at producing new weapons "clearly directed against the people of the Soviet Union". His reply to the Association after its decision had been announced also included these words: "Your council is attempting to set up the precedent that official science will not tolerate any serious criticism of essentially political doctrines. This clearly justifies the contention in my speech that the use of science for the preparation of war is a first step to destroying its freedom."

The next issue of DISCOVERY will contain a title-page and index for Volume X. Orders for binding cases for Volume X can now be accepted, price 4s. post free. We shall be pleased to bind readers' copies, supplying index, title-page and binding case, at an inclusive charge of 9s. 6d. Bound Volumes for 1949 will be available in the New Year, Price 30s.

Guano Islands

A LECTURE on the islands where guano (the phosphate manure, consisting mainly of calcium and tricalcium phosphate, produced by birds such as cormorants) was given to the Linnean Society on October 13 by Prof. G. E. Hutchinson of Yale University. He said that prior to total exploitation in the mid-nineteenth century, the principal guano islands of the Peruvian coast bore deposits of consolidated bird-droppings up to about 50 metres in thickness. The archaeological evidence assembled by Professor G. A. Kubler and the stratigraphic evidence brought together by the speaker indicate that this huge accumulation could have formed in a few thousand years. Prior to initiation of deposition on the Peruvian coast there was certainly much guano being laid down on the coast of Chile, but farther north where the main deposits are formed today it is probable that the prevailing climatic regime was that of modern 'abnormal' wet years. In the central Pacific phosphatic guano deposits of post-glacial date and of large size, exceeding 10,000 tons, are confined to the area of low rainfall along the equator. There is, however, reason to suppose that in such a region deposits of the size implied by the phosphate beds of Nauru, Ocean Island and some other localities in the Pacific, Christmas Island in the Indian Ocean and some West Indian Islands, could not have formed in a reasonable time. It is suggested that such huge deposits originated in periods of greatly enhanced oceanic fertility, due to a lowering of the vertical stability of the oceans when the supply of cold water was reduced during interglacials.

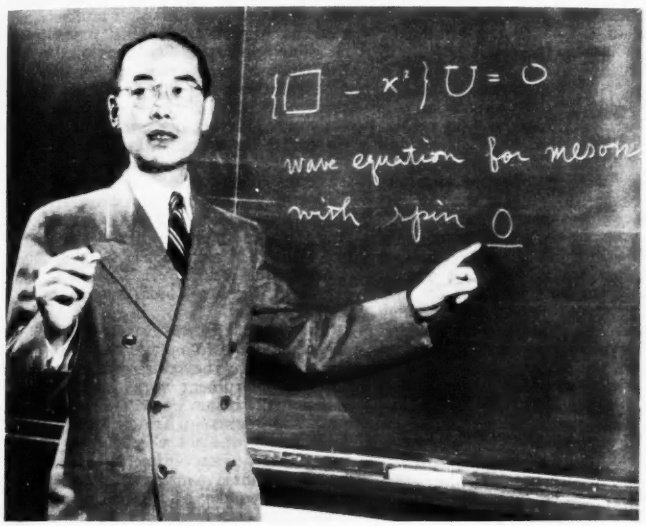
Potash in Yorkshire

IN the Report of the Mineral Development Committee (H.M.S.O., 1949) the potentialities of potash deposits in north Yorkshire are discussed with an urgent enthusiasm not usually found in such documents.

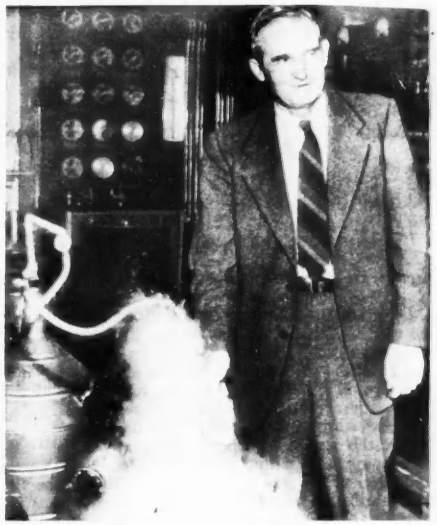
Until World War I potash was virtually a German monopoly. The first peace gave France a control over some of the former German mines; also, America, after experiencing acute potash shortages during the war, located her own potash deposits and quickly developed a self-sufficient potash industry. Towards the time of the second war, the concentrated solution of the Dead Sea was being used to build up a new potash industry in Palestine, and much of the potash used by British farmers during the 1939–45 war came from this source. Since the war, however, Britain has reverted to dependence upon the European potash mines. This has meant importing from French-controlled mines, from the Western Zones of Germany, and from the Russian Zone.

Just before the war potassic minerals were found at great depths at Aislaby, near Whitby, Yorkshire. An exploratory bore-hole, sunk in search of oil, had located these deposits in beds at an approximate depth of 4000 ft. At a little short of 5000 ft. the salt beds give way to a saturated brine solution which contains sodium and potassium chlorides in a ratio of about 11 to 1. Proposals to develop these resources have hitherto been rejected as uneconomic; even as an emergency during World War II they went unused. Geological evidence suggests that the Yorkshire deposits are closely related to the rich north German deposits, accumulated by the evaporation of the same sea which once stretched from Germany to eastern England.

A second bore-hole, opened in 1948 and carried to a depth of 5500 ft., has produced much more promising information about the solid deposits. Beds of potassic minerals of substantial thickness have now been indicated. The evidence obtained from the original oil-prospecting bore-hole had indicated that the layer of 'sylvinite', the most suitable potassic mineral, was disappointingly thin. The Report of 1949 now refers to "the occurrence of a substantial quantity of a mineral highly valuable to agriculture" and of "the prospect emerging of a most important addition to the country's natural resources." The report states it is urgently necessary to sink further bore-holes to determine the magnitude and economic



1949 NOBEL PRIZEWINNERS. *Prof. Hideki Yukawa, awarded the Physics Prize, is the first Japanese ever selected in the 49-year history of the awards.*



Prof. W. F. Giauque, winner of the Chemistry Prize.

value of the deposits and to ascertain the most suitable method of development. For Britain such a development would undoubtedly be the most important 'fertiliser event' since the commencement of nitrogen fixation at Billingham in 1919.

1949 Nobel Prizes

THIS year's Nobel Prizes for Physics and Chemistry have been awarded to Prof. HIDEKI YUKAWA and Prof. WILLIAM FRANCIS GIAUQUE respectively.

Prof. Yukawa's outstanding contribution to physical science was made in 1935, when he was 28 years old and a lecturer at Osaka University. He postulated the existence of a new nuclear particle, the meson, and his hypothesis was confirmed in 1938 when Anderson and Neddermeyer, working in the U.S.A., proved the existence of the meson experimentally. Yukawa is now visiting professor of theoretical physics at Columbia University, New York.

Prof. Giauque, who is 54 and on the staff of the University of California, gains his award for his studies of the behaviour of matter at temperatures approaching absolute zero.

The Nobel Prize for physiology and medicine is shared by Dr. Walter Rudolf Hess, director of Zürich University Institute of Physiology since 1917, and Professor Antonio Egas Moniz, of Lisbon University. Dr. Hess is an authority on the physiology of the eye and the brain. Prof. Moniz was responsible in 1935 for introducing the leucotomy operation, in which the fibres connecting the frontal lobes of the brain and the thalamus are cut. The operation has been tried in many countries, and it has improved the condition of many mental cases.

The Nobel Peace Prize for 1949 goes to Lord Boyd-Orr, formerly director of the United Nations Food and Agriculture Organisation.

Guide to Broadcasting Stations

THE fifth edition of *Guide to Broadcasting Stations*, compiled by the staff of *Wireless World* and published at 1*s.* 6*d.* by Iliffe and Sons Ltd., gives present operating details about Europe's 400 long- and medium-wave stations. It also allows for the changes of wavelength agreed at Copenhagen which come into force in March 1950. Operating details of nearly 1300 short-wave stations are given in tabular form.

In addition, this booklet includes details of Europe's television and E.H.F. broadcasting stations, and special service stations—such as meteorological and standard frequency transmitters; world time constants; revised list of international call signs; and wave-length-frequency conversion table.

Prize for Popular Science Book

WITH his book *A Direct Entry Into Organic Chemistry* Professor John Read of St. Andrews University has been awarded the million lire Cortina Prize presented by the Italian journal *Ulisse* for the best popular scientific book published in the last five years in any country. Professor Read's book is published in the Methuen Home Study series, at 4*s.* 6*d.*

The Brabazon's Engines

SEVERAL readers have written to point out that the Brabazon (of which a photograph appeared in the October issue) is not jet-propelled. The prototype Brabazon has in fact eight Bristol Centaurus piston engines mounted in coupled pairs. The second Brabazon to be built will have Bristol Proteus turbo-airscrew engines.

Scientific Collaboration in Africa

AFTER ten days' deliberation in six separate Sections, the hundred delegates to the African Regional Scientific Conference, held in Johannesburg during the latter half of October, unanimously supported a proposal for the establishment of an advisory Scientific Council for Africa south of the Sahara. The proposed Council, which will come into being only after the Governments concerned approve of the scheme, will consist of a small body of scientists representative of the various sciences and of the different territories and will be directed by a Chairman who should be a scientist thoroughly conversant with African problems and able to devote his whole time to the work of the Council. The Council would not itself establish research institutions or laboratories but would operate through the facilities available, or to be established, within the various territories. Its functions would be consultative and advisory, and it would endeavour to co-ordinate research work on related subjects in different areas so as to minimise duplication of effort. One of its most important duties would be to see that working scientists have ready access to information concerning published material and work in progress. It would also be able to advise Governments on the need for new developments or the establishment of new research institutes, but the setting up of such institutes will be left to the territories themselves.

There will thus be no lack of work for the Scientific Council when it comes into being, and it is to be hoped that its formation will not long be delayed. Africa has great potentials but cannot be properly developed until a great deal more basic scientific work has been accomplished. The new Council offers the best prospect that this will be achieved.

Night Sky in December

The Moon.—Full moon occurs on Dec. 5d 15h 13m, U.T. and new moon on Dec. 19d 18h 55m. The following conjunctions with the moon take place:

December.

12d 23h	Saturn in conjunction with the moon	Saturn	0°·3	S.
13d 09h	Mars „	Mars	0·6	N.
20d 23h	Mercury „	Mercury	3	N.
22d 08h	Jupiter „	Jupiter	4	N.
22d 23h	Venus „	Venus	4	N.

In addition to these conjunctions with the moon, Venus is in conjunction with Jupiter on Dec. 7d 03h, Venus being 2° S.

The Planets.—Mercury is too close to the sun for favourable observation in the early portion of the month but in the middle and end of the month the planet sets ½h and 1h 20m after sunset, respectively, and can be seen for a short period in the western sky especially towards the end of the month. Venus can be seen as an evening star, setting at 19h, on Dec. 1 and 19½h at the middle and end of Dec. It is interesting to notice that the times of setting throughout the month remain nearly the same. Mars rises about midnight in the early part of the month but is not a conspicuous object. Jupiter sets at 19h 36m, 18h 56m, and 18h 13m, at the beginning, middle and end of the month, respectively, and is easily recognised fairly low in the western sky, stellar magnitude −1·6, which is nearly as bright as Sirius but only one-fifteenth the brightness of Venus during December. Saturn rises about 10 minutes after midnight on Dec. 1 and at 22h 15m at the end of the month, and can be seen late at night and in the morning hours in the constellation of Leo and a little west of Virgo.

In the November issue I mentioned about the Great Nebula in Orion which is just visible to the naked eye on a clear night. This nebula is about 600 light-years distant and at its greatest width is about 13 light-years in diameter; in other words, its diameter is three times the distance between the earth and the nearest star, Proxima Centauri. It is gaseous but so attenuated that the most perfect vacuum produced by a modern air pump is a million times more dense than its gases. It does not shine by its own light but absorbs the radiation of stars embedded in it and then re-emits this radiation at a different wave-length which renders it visible to us. The star or stars responsible for this phenomenon must have a high temperature—very much higher than that of the sun, which is about 6000° C—and we could not see the nebula if only stars like the sun were embedded in it.

M.D.

Mr. W. G. Pye

THE death occurred in October, 1949 at the age of 79 of W. G. Pye, whose name was so well known in connexion with the Cambridge radio works and the Cambridge Scientific Instrument Company.

W. G. Pye, who was trained at Elliot's scientific instrument factory in London, went to Cambridge to succeed Mr. A. T. Bartlett (later Head of the Research Department of the English Electric Company) as superintendent of the Cavendish Laboratory Workshops in 1892, where he remained until 1899. It is probable that no laboratory mechanic has ever had such a close association with fundamental physics, and made so much entirely new apparatus in such a short time. He had as his chief J. J. Thomson. 'J.J.' was not a scientist who made his own apparatus; indeed he was so clumsy with his fingers that his staff and research students discouraged him from even handling apparatus. So it was that W. G. Pye came to make the first cathode-ray tube and the X-ray tubes for J. J. Thomson's momentous discovery of the electron. He worked during his seven years at the Cavendish for a host of famous scientists —including C. Chree, noted for his work on terrestial magnetism; Sir Napier Shaw, for many years Director of the Meteorological Office; G. F. C. Searle, Cambridge's most famous teacher of practical physics; Sir William Dampier, H. L. Callendar, Lord Rutherford, J. S. E. Townsend, Paul Langevin, the late Lord Rayleigh, and J. Zelany.

In 1899 he left the Cavendish to go into the commercial manufacture of scientific instruments. The establishment of a scientific instrument works in Cambridge was actually begun by A. G. Dew-Smith, who had rooms in Trinity College, not by right (he was not a Fellow nor holder of a University appointment), but because he was 'business manager' of the School of Physiology under Michael Foster. Dew-Smith realised how much time and money was lost because the Physiological Laboratory was not able to buy or, at that time, even make standard and commonplace pieces of apparatus, but had to have everything made to order elsewhere. The Laboratory having no funds to equip a workshop or pay for a skilled mechanic, Dew-Smith proceeded to start a workshop himself with his own finance in St. Tibbs Row and engaged a very skilled mechanic —W. G. Pye's father. The workshop flourished, and from making simple apparatus it branched out into the more elaborate. The greater knowledge of science and engineering now needed was supplied by Mr. (later Sir Horace) Darwin. Father Pye, who, according to 'J.J.' "was a bit of a character as well as a good workman; he held very decided views about most things, including the merits of those for whom he was making apparatus. He expressed these views quite freely, to the great delight and amusement of his master", was joined by his son who specialised in making scientific apparatus for use in schools. Later wireless sets were added to the products, developing into the separate firm of Pye Limited.

This ultrasonic soldering iron, made by Mullard Electronic Products and shown at the Scientific Instrument Manufacturers' Association's exhibition, solves the problem of soldering aluminium and light alloys. The oxide film that forms on these metals makes ordinary soldering impossible, but it is disrupted by the vibration set up by this tool. The soldering bit is heated by a conventional resistance winding.

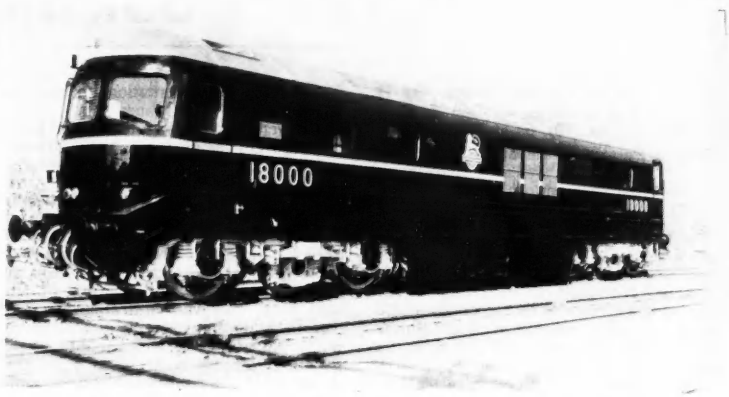
Science Laboratories on Show to Public

BY permission of the departmental heads, the Association of Scientific Workers was able to give the public some idea of the type of work which is in progress in the Physics Department and Electrical Engineering Department of Birmingham University on September 23–24. The outstanding instruments shown were, of course, Professor Oliphant's cyclotron and synchrotron; but due to the impracticability of conducting large crowds round the machine enclosures, these two machines themselves could only be shown to selected parties from local schools. Other visitors had the operation of the electron accelerators explained with the aid of scale models, and they also saw a 250 kilovolt Van der Graaf electrostatic generator. Artificial radioactivity in silver was demonstrated by exposing

silver coins to neutron radiation from a radium-beryllium source; and this is in fact a quick method of estimating the silver content of an alloy, as shown by the varying responses from coins of different dates, ending with nil from the present-day cupro-nickel coins. The usual electronic counting circuits were in general use with Geiger radiation detectors to count the number of radiated particles, and the deflection of beta rays by a magnetic field was demonstrated.

The University Observatory was open, giving visitors a view of an astronomical telescope and the precise mechanical devices needed to keep it trained on a fixed point in the sky while the earth rotates, and by contrast with this example of long-range visible optics there was also a display of photographs obtained with the electron microscope now installed in the university.

A tour of the Electrical Engineering Department showed a number of research projects related to the study of automatic control (or 'servo') systems.



British Railways' first gas-turbine locomotive, which will be operating next year. Ordered in 1946 by the Great Western Railway, it has been constructed by the Swiss firm Brown-Boveri. A British gas-turbine locomotive is under construction by Metropolitan Vickers.

The Unesco Conference

THE fourth session of the General Conference of Unesco which was held in Paris in October was originally designed to last only one week and to be 'a business meeting.' It lasted two weeks and the term 'business' was very much broadened.

In fact, this fourth session was a preparatory trial of strength for the new Director-General, Dr. Torres Bodet, who was meeting officially the various member states of Unesco for the first time. He 'won through'; but mainly because key delegations—such as the British—were not prepared to do anything which might hinder the development of Unesco, and because of the devaluation.

The fact is that Unesco has been under fire because during its three years of existence it has talked much, and has rather tended to publicise itself on the basis of well-meaning but airy projects. For example, in the field of the Natural Sciences, the Hylean-Amazon project, received much publicity. But this has now foundered on the rocks of political mistrust and suspicion.

From the very first day of the session, the British delegation, headed by Mr. David Hardman, M.P., Parliamentary Secretary to the Ministry of Education, insisted that Unesco had to be practical and business-like, and that it had to concentrate its programme so as to secure the best results. The British particularly pressed for a ceiling for the 1950 budget. They were supported by the Commonwealth countries, but opposed in the main by the French and the smaller countries. Finally, an American suggestion was adopted that there should be a target figure of eight million dollars. This meant, in effect, that the final budget figure agreed on was this sum, and when this was announced, the British delegation voted for it, thus maintaining unanimity within Unesco.

The announcement of devaluation of the British currency helped, because it meant that fewer dollars could now go further in terms of devalued currencies. As a result of this, it is interesting to note that contracts now being issued by Unesco for individuals in countries outside the U.S.A. will no longer be in dollars but in terms of the native currency.

The Director-General at all stages had the support of the 'have-not' countries, which feel that Unesco is making an important contribution to their well-being, and these constitute a formidable block.

The ideological split between the East and the West was also expressed clearly during this session. At one stage, the Czech, Polish and Hungarian delegations left the conference room and remained outside for several days. The occasion was the debate on Unesco work in Germany. The Eastern states wanted this question to be held over until Unesco meets in Florence next May. They insisted that there was a rebirth of militarism, of nationalism and of Hitlerism in western Germany, and that Unesco could not compromise itself by working with such elements.

The opposition viewpoint, as expressed by Switzerland, the U.S.A., India and Denmark was that because of this rebirth of nationalism Unesco must increase its activities within Germany. Finally, a compromise acceptable to the majority was reached in an amendment giving the Director-General of Unesco the right to stop activities in Germany if any campaigns favouring ideas contrary to those of Unesco were developed by leading Germans in the fields of education, science and culture.

The programme of work proposed by the Director-General was, in the main, adopted. As far as the Natural Sciences Department—under Professor Pierre Auger—is concerned, there was general recognition that its activities were important. In Abstracting, the Director-General was instructed to aid the development of the indexing, abstracting and reviewing of scientific literature by promoting international collaboration, co-ordination and organisation in these fields; and to that end: to convene committees on the abstracting of various sciences, including, jointly with the World Health Organisation, the Co-ordinating Committee on the Abstracting and Indexing of Medical and Biological Sciences; to publish a directory of science indexing and abstracting services; to publish a consolidated list of the periodicals which are indexed and abstracted by the various science indexing and abstracting services. In Engineering, to convene a conference of delegates of international non-governmental engineering science organisations to consider the establishment of an Advisory Committee on International Engineering Science Congresses taking into account the recommendations of the Expert Committee convened in 1949. The Director-General was also instructed to circulate to Member States the recommendations of the Committee of Experts called in 1949 on the proposal to set up an International Institute of the Arid Zone, inviting their comments, and, thereafter, if it appears desirable, to call a conference of delegates of interested countries and Specialised Agencies to discuss the possible establishment of such an Institute. He was also directed to circulate the proposal to set up an International Computation Centre and to submit a report to a future session.

In the discussion on the Popularisation of Science, Professor David Brunt, and the American, Dr. Gross, said that they were in favour of some large financial cuts in the programme. This did not, however, occur, for the discussion revealed that the smaller countries took an extremely favourable viewpoint of these activities. An interesting point was raised by M. Jean Piagét of Switzerland, who said that he disliked the French term 'vulgarisation' very much and would prefer to see another used. In fact, the term 'diffusion' is now beginning to be used in its place.

Support was again given to the annual Unesco discussion theme which is to be "World Energy and Power Resources", for 1950/51.

The Bookshelf

The Origins of Modern Science. By H. Butterfield (London, G. Bell & Sons, 1949, 217 pp., 10*s.* 6*d.*).

Science Past and Present. F. Sherwood Taylor (London, William Heinemann, 2nd (enlarged) edition, 1949, 368 pp., 12*s.* 6*d.*).

THE history of science has been receiving more attention lately from both historians and scientists, although with perhaps less unanimity of outlook than may be expected when more of the necessary spadework has been undertaken. Prof. Butterfield's book originates directly from the wish of the History of Science Committee at Cambridge to see a course of teaching established which "should deal, not merely with the background of science, but with the history of scientific activity itself, and this not merely in occasional episodes but as the study of a continuous process of development". It is of particular interest that this programme should have been entrusted to a general historian, and even more to watch the impact of science on Prof. Butterfield. Whatever other criticisms might be made—and they are more of the state of the history of science than of the author—he cannot be accused of under-rating his subject. He asserts roundly that the scientific revolution "reduces the Renaissance and Reformation to . . . mere episodes" and as he wrote, may well have thought of that long-sustained duel between Belloc and Coulton. It could be argued that the birth of modern science should be regarded, not in antithesis, but as an aspect and continuation of the Renaissance to which too little attention has so far been given, whether in relation to the Renaissance period proper, or in its general context as part of history. In time, Prof. Butterfield covers the period from 1300 to 1800; and in content deals with the downfall of the Aristotelian and Ptolemaic systems, gravitation, the development of the experimental method, the postponed revolution in chemistry, and ideas of progress and evolution. As a historian, he emphasises repeatedly the need to look at the past through the spectacles of the past; and this, together with his wide approach, is probably the chief value of his contribution—apart from its immediate purpose of providing the Cambridge course already mentioned. It is to be hoped that his book will be revised and kept to date in the light of research and criticism: for it is no finished study, and does not claim to be.

By contrast, Dr. Sherwood Taylor's book, now issued in enlarged edition, is a source book illuminated by narrative for continuity. It is unique of its kind, and would be invaluable if for no other purpose than as a collection of relevant quotations from original sources. The additional chapters relate to atomic theory and modern physics, and their historical origin in the study of gases. In time, the book ranges from ancient Egypt to the present day.

A. W. H.

Technology and International Relations. By William F. Ogburn and others. (University of Chicago Press and Cambridge University Press, 1949, 202 pp., 22*s.* 6*d.*).

IN his famous lectures on political science given towards the end of the nineteenth century at Cambridge, the historian Seeley, defining the province of history as those facts and events which affect the state, asserted that "government and legislation, and wars and alliances, concern the state, but scientific discoveries do not". No one living in the middle of the twentieth century is likely to share this view; and if any doubt remained, it would soon be dispelled by the growing number of books devoted to the study of science and technology as factors of social and political history, which have appeared in recent years.

This latest book is the collection of the Harris Foundation Lectures on *Technology and International Relations*, given by a number of sociologists at the University of Chicago in 1948. Four of these lectures deal with the influence on international relations of steam and steel, aviation, atomic energy, and the mass communication inventions. The other four study, in turn, the process of adjustment to new inventions, the growth of political areas as affected by the progress of technology, the influence of new techniques of war on national policies, and the impact of modern technology on the world order.

There is much in this book that is unexceptionable, but relatively little that is new, original or striking. It is handicapped by the usual disadvantages of symposiums—lack of unity and unevenness of quality. Most of the lectures are either too short and general to give an exhaustive view of their subject, or too long and detailed to reveal a general trend or outline unblurred by incidentals. There are, however, exceptions; Mr. Bernard Brodie's study on "New techniques of war and national policies", which deals to a large extent with the political implications of atomic energy, achieves a neat balance between these two extremes, besides giving a clear and valuable exposition on atomic-age strategy. There are in this short lecture many points of prime importance which are conspicuously absent in some major works on the political consequences of atomic energy.

The lectures are concerned with how inventions and science affect international relations, but the authors are well aware of the fact that international relations in their turn affect science and invention; that they were an important factor in developing the submarine, for instance. As Professor Quincy Wright points out in his study on "Modern Technology and the World Order", inventions and opinions influence one another, and the effect of an invention depends upon the values and the culture of the existing social order, just as the effect of opinions or ideologies depends in considerable degree upon the means for their realisation which are made available by science and technology.

The whole subject of technology and international relations is one in which there has not been much scientific research, and this perhaps explains some of the gaps which this book leaves. But it should be remembered that this set of lectures is in the nature of a pioneering venture and as such makes valuable reading, especially in view of the paucity, in English at any rate, of serious studies on the subject.

E. M. F.

The Floral Year. By L. J. F. Brimble. (London, Macmillan, 1949, 622 pp., 30*s.*).

PLENTY of botanical textbooks are published, but there are very few authors writing books about plants that can be read with both profit and enjoyment by general readers. One of the rare exceptions is L. J. F. Brimble, who used to be a university lecturer until he joined *Nature* and became joint editor of that unique journal. *The Floral Year* is one of a trilogy: its predecessors were *Flowers in Britain* and *Trees in Britain*. This book describes the pageant of the flowers month by month as they bloom. The arrangement of the subject in this way gives a book that readers will want to dip into time and time again, and it is likely to be used as constantly as are those gardening books that are divided up in the same sort of way. The book is profusely illustrated with photos (of which there are several hundred), with 25 special colour plates by W. Reeves and with many excellent detail drawings by the author. A most readable and pleasing book which would make an ideal Christmas present.

Burma's Icy Mountains. By F. Kingdon-Ward (London, Jonathan Cape, 1949, 287 pp., 15*s.*).

THIS book is the account of two expeditions into the mountainous frontiers of Northern Burma.

The book should be read by every second-year biology student for the introduction that it gives to plant geographical problems as seen in the field, and the way it assists the reader to grasp the inhospitable character of wild forest vegetation, whether of Burma directly, or of undrained prehistoric British woodland by inference. The significance of predatory tribes in prehistory, and the manner in which, unwittingly, man has initiated soil erosion all come out brilliantly in casual passages.

According to the publishers this account is intended for the general reader, but Capt. Kingdon-Ward in his preface seems to suspect that such may be too much to hope for. And unless the general reader be a well-informed gardener, and of persisting patience as a reader, it is likely that it is the author who will be right. But the scientific reader will find much of the book to be of considerable interest, though he will often be irritated by weak and unfinished descriptions, disconnected treatment and repetitions.

M. H. C.

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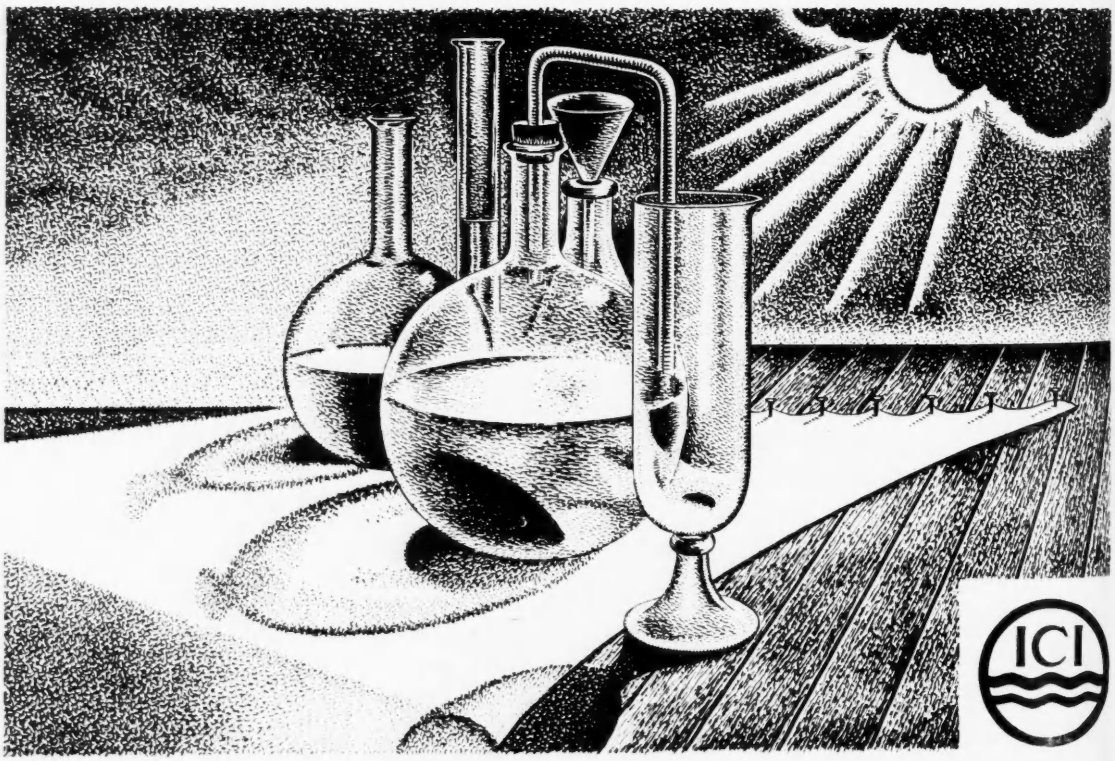

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